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JAMES STIRLING

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affections of the  
No doubt you know what a generall change of the people of England the  
late proceedings hath occasioned; the mobs begun on the 28 of May to pull  
down meeting houses and whiggs houses, and to this very day they con-  
tinue doing the same, the mob at in Yorkshire and Lancashire as-  
mounted to severall thousands, and would have beat of the forces sent  
against them had they not been dissuaded by the more prudent sort.  
and they are now raging in Coventry and Daintry: so as the court saith  
the nation is just ripe for a rebellion. There were severall houses of  
late at London searched for the Chevalier, the D. of Berwick and Mr  
Lesly. Oxford is impeached of high treason and high crimes and misde-  
manners, and is now in the tower, a little while ago both whiggs &  
Tories wished him hanged, but he now gained some Tories to stand  
his friends in opposition to the Whiggs. They cant make out e-  
nough to impeach the rest they designed. I had a letter from North-  
brooke lately, I shall delay an answer till I have the occasion  
of a frank. My cousin James sent me a letter the other day  
from Amsterdam, he is just come from the Canaries and designs to  
return there without coming to Britain, he remembers himself very  
kindly to you and all friends with you. I give my humble duty to  
you and my mother and my kind respects to my brothers sisters and all  
my relations I am  
Yr

Your most dutifull son J<sup>a</sup> Stirling

# JAMES STIRLING

A SKETCH OF HIS

*Life and Works*

ALONG WITH HIS

SCIENTIFIC CORRESPONDENCE

BY

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EDINBURGH UNIVERSITY

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TO THE MEMORY OF  
JOHN STURGEON MACKAY, LL.D.  
TO WHOSE INSPIRATION IS LARGELY DUE  
MY INTEREST IN THE  
HISTORY OF MATHEMATICS



## PREFACE

THE Life of Stirling has already formed the subject of a very readable article by Dr. J. C. Mitchell, published in his work, *Old Glasgow Essays* (MacLehose, 1905). An interesting account of his life as manager of the Leadhills Mines is also given by Ramsay in his *Scotland and Scotsmen in the Eighteenth Century*.

The sketch I here present to readers furnishes further details regarding Stirling's student days at Balliol College, Oxford, as culled from contemporary records, along with more accurate information regarding the part he played in the Tory interests, and the reason for his departure for Italy. Undoubtedly, when at Oxford, he shared the strong Jacobite leanings of the rest of his family. Readers familiar with Graham's delightful *Social Life in Scotland in the Eighteenth Century*, and the scarcity of money among the Scottish landed gentry, will appreciate the tone of the letter to his father of June 1715, quoted in full in my sketch.

Whether he ever attended the University of Glasgow is a moot point. Personally, I am inclined to think that he did, for it was then the fashion to enter the University at a much earlier age than now, and he was already about eighteen years of age when he proceeded to Oxford.

Very little is known regarding his stay in Venice and the date of his return to Britain; but his private letters show that when he took up residence in London he was on intimate terms of friendship with Sir Isaac Newton and other distinguished scholars in the capital.

I have taken the opportunity here to add—what has hitherto not been attempted—a short account of Stirling's published works, and of their relation to current mathematical thought. In drawing up this account, I had the valuable

assistance of Professor E. T. Whittaker's notes on Part I of Stirling's *Methodus Differentialis*, which he kindly put at my disposal.

Stirling's influence as a mathematician of profound analytical skill has been a notable feature within the inner circle of mathematicians. Witness, for example, the tribute of praise rendered by Laplace in his papers on Probability and on the Laws of Functions of very large numbers. Binet, in a celebrated memoir on Definite Integrals, has shown Stirling's place as a pioneer of Gauss. Gauss himself had most unwillingly to make use of Stirling's Series, though its lack of convergence was anathema to him. More recently, Stirling has found disciples among Scandinavian mathematicians, and Stirling's theorems and investigations have been chosen by Professor Nielsen to lay the foundation of his Monograph on Gamma Functions.

The Letters, forming the scientific correspondence of Stirling herewith published, make an interesting contribution to the history of mathematical science in the first half of the eighteenth century. I have little doubt that suitable research would add to their number. I have endeavoured to reproduce these as exactly as possible, and readers will please observe that errors which may be noted are not necessarily to be ascribed to negligence, either on my part or on that of the printer. For example, on page 47, the value of  $\pi/2$  given by De Moivre's copy of Stirling's letter (taken from the *Miscellanea Analytica*) is not correct, being 1.57079632679, and not 1.5707963279 as there stated.

A few notes on the letters have been added, but, in the main, the letters have been left to speak for themselves.

I am deeply grateful for the readiness with which the Garden letters were placed at my disposal by Mrs. Stirling, Gogar House, Stirling. I am also indebted to the University of Aberdeen for permission to obtain copies of Stirling's letters to Maclaurin.

In the troublesome process of preparing suitable manuscript for the press, I had much valuable clerical assistance from my sister, Miss Jessie Tweedie.

Of the many friends who have helped to lighten my task I am particularly indebted to Dr. C. G. Knott, F.R.S., and to Professor E. T. Whittaker, F.R.S., of Edinburgh University; also to Professor George A. Gibson, of Glasgow University, who gave me every encouragement to persevere in my research, and most willingly put at my disposal his mature criticism of the mathematicians contemporary with Stirling.

Facsimile reproductions of letters by James Stirling and Colin Maclaurin have been inserted. These have never before appeared in published form, and will, it is hoped, be of interest to students of English or Scottish history, and to mathematical scholars generally.

The heavy cost of printing during the past year would have made publication impossible but for the generous donations from the contributors mentioned in the subjoined list of subscribers, to whom I have to express my grateful thanks.

CHARLES TWEEDIE.

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## FACSIMILES

Facsimile of last page of Letter by

STIRLING to his FATHER, 1715 (pages 6-7)    *Frontispiece*

Facsimile of last page of Letter by

MACLAURIN to STIRLING, 1728 (Letter No. 1)

*facing p. 57*



COAT OF ARMS OF THE STIRLINGS OF GARDEN.

## LIFE OF JAMES STIRLING

JAMES STIRLING, the celebrated mathematician, to whose name is attached the Theorem in Analysis known as Stirling's Theorem, was born at Garden in the county of Stirling, Scotland, in 1692. He was a member of the cadet branch of the Stirling family, usually described as the Stirlings of Garden.

The Stirling family is one of the oldest of the landed families of Scotland. They appear as proprietors of land as early as the twelfth century. In 1180, during the reign of William the Lion, a Stirling acquired the estate of Cawder (Cadder or Calder) in Lanarkshire, and it has been in the possession of the family ever since. Among the sixty-four different ways of spelling the name Stirling, a common one in those early days, was a variation of Striveling.

In 1448, the estate of Keir in Perthshire was acquired by a Stirling. In 1534 or 1535 these two branches of the family were united by the marriage of James Striveling of Keir with Janet Striveling, the unfortunate heiress of Cawder. Since that time the main family has been, and remains, the Stirlings of Keir and Cawder. By his second wife, Jean Chisholm, James had a family, and of this family Elizabeth, the second daughter, married, in 1571-2, John Napier of Merchiston, the famous inventor of logarithms, whose lands in the Menteith marched with those of the Barony of Keir. This was not the first intermarriage between the Napiers and the Stirlings, for at the former Napier residence of Wright's Houses in Edinburgh (facing Gillespie Crescent), there is preserved a stone the armorial bearings on which record the marriage of a Napier to a Stirling in 1399.

Early in the seventeenth century Sir Archibald Stirling of Keir bought the estate of Garden, in the parish of Kippen (Stirlingshire), and in 1613 he gave it to his son (Sir) John Stirling, when Garden for the first time became a separate

estate of a Stirling. The son of John, Sir Archibald Stirling, was a conspicuous Royalist in the Civil War, and was heavily fined by Cromwell; but his loyalty was rewarded at the Restoration, and he ascended the Scottish bench with the title of Lord Garden. Lord Garden, however, succeeded to the estate of Keir, and his younger son Archibald (1651–1715) became Laird of Garden in 1668.

Archibald's eventful career is one long chapter of misfortunes. Like the rest of the Stirlings he adhered loyally to the Stuart cause. In 1708, he took part in the rising called the Gathering of the Brig of Turk. He was carried a prisoner to London, and then brought back to Edinburgh, where he was tried for high treason, but acquitted. He died in 1715, and thus escaped the penalty of forfeiture that weighed so heavily on his brother of Keir. He was twice married. By his first wife he had a son, Archibald, who succeeded him, and by his second marriage, with Anna, eldest daughter to Sir Alexander Hamilton of Haggis, near Linlithgow, he had a family of four sons and five daughters. James Stirling, the subject of this sketch, and born in 1692, was the second surviving son of this marriage. (The sons were James, who died in infancy; John, who acquired the Garden estate from his brother Archibald in 1717; James, the mathematician; and Charles.)

The Armorial Bearings of the Garden<sup>1</sup> branch of the Stirlings are:

*Shield:* *Argent* on a Bend *azure*, three Buckles *or*: in chief, a crescent, *gules*.

*Crest:* A Moor's Head in profile.

*Motto:* Gang Forward.<sup>2</sup>

## YOUTH OF STIRLING

### OXFORD

SAVE for the account given by Ramsay of Ochtertyre (*Scotland and Scotsmen, from the Ochtertyre MSS.*), which is not trustworthy in dates at least, little is known of the early

<sup>1</sup> Garden, pronounced Gardén, or Gardenne.

<sup>2</sup> Gang forward; *Scoticé* for *Allez en avant*.

years and education of Stirling, prior to his journey to Oxford University in 1710.

Ramsay, it is true, says that Stirling studied for a time at Glasgow University. This would have been quite in accordance with Stirling tradition, for those of the family who became students had invariably begun their career at Glasgow University; and the fact that Stirling was a Snell Exhibitioner at Oxford lends some colour to the statement. But there is no trace of his name in the University records. Addison, in his book on the Snell Exhibitioners, states that 'Stirling is said to have studied at the University of Glasgow, but his name does not appear in the Matriculation Album'.

From the time that he proceeds on his journey to Oxford his career can be more definitely traced, though the accounts hitherto given of him require correction in several details. Some of the letters written by him to his parents during this period have fortunately been preserved. This fact alone sufficiently indicates the esteem in which he was held by his family, and their expectation of a promising future for the youth. In one of these he narrates his experiences on the journey to London, and his endeavour to keep down expenses: 'I spent as little money on the road as I could. I could spend no less, seeing I went with such company, for they lived on the best meat and drink the road could afford. Non of them came so near the price of their horses as I did, altho' they kept them 14 days here, and payed every night 16 pence for the piece of them.' He reached Oxford towards the close of the year 1710. He was nominated Snell Exhibitioner on December 7, 1710, and he matriculated on January 18, 17 $\frac{10}{11}$ , paying £7 caution money. On the recommendation of the Earl of Mar he was nominated Warner Exhibitioner, and entered Balliol College on November 27, 1711. In a letter to his father of the same year (February 20, 1711) he gives some idea of his life at Oxford: 'Everything is very dear here. My shirts coast me 14 shillings Sterling a piece, and they are so course I can hardly wear them, and I had as fit hands for buying them as I could.' . . . 'We have a very pleasant life as well as profiteable. We have very much to do, but there is nothing here like strickness. I was lately matriculate, and with the help of my tutor I escaped the oaths, but with much ado.'

He thus began academic life at Oxford in good spirits, but as a non-juring student. At this period Oxford University was not conspicuous for its intellectual activity. The Fellows seem to have led lives of comfortable ease, without paying much regard to the requirements of the students under their care.

As we shall see in Stirling's case, the rules imposed upon Scholars were very loosely applied, and, naturally, complaint was made at any stringency later. At the time we speak of political questions were much in the thoughts of both students and college authorities. The University had always been faithful to the house of Stuart. It had received benefits from James I.

For a time Oxford had been the head-quarters of King Charles I during the Civil War, and his cavaliers were remembered with regret when the town was occupied by the Parliamentary forces, and had to endure the impositions of Cromwell. At the time of Stirling's entry the reign of Queen Anne was drawing to a close. Partisan feeling between Whigs and Tories was strong, and of all the Colleges Balliol was most conspicuously Tory. According to Davis (*History of Balliol College*) Balliol 'was for the first half of the 18th century a stronghold of the most reactionary Toryism', and county families, anxious to place their sons in a home of sound Tory principles, naturally turned to Balliol, despite the fact that Dr. Baron, the Master, was a stout Whig. It is, therefore, abundantly clear that Stirling had every reason to be content with his political surroundings at Balliol, with what results we shall see presently. Perhaps the best picture of the state of affairs is to be gathered from the pages of the invaluable *Diary of T. Hearne*, the antiquarian sub-librarian of the Bodleian. For Hearne all Tories were 'honest men', and nothing good was ever to be found in the 'vile Whiggues'. His outspoken Tory sentiments led to his being deprived of his office, and almost of the privilege of consulting books in the Library, though he remained on familiar terms with most of the resident Dons.

Luckily for us, James Stirling was one of his acquaintance, and mention of Stirling's name occurs frequently enough to enable us to form some idea of his career. Doubtless

their common bond of sympathy arose from their Tory, nay their Jacobite, principles, but it speaks well for the intellectual vigour of the younger man that he associated with a man of Hearne's scholarship. Moreover, Stirling must have been a diligent student, or he could never have acquired the scholarship that bore its fruit in 1717 in the production of his *Lineae Tertii Ordinis*, a work which is still a recognized commentary on Newton's *Enumeration of Curves of the Third Order*. But he was not the sort of man to be behindhand in the bold expression of his opinions, and he took a leading part among the Balliol students in the disturbances of 1714-16.

The accession of George of Hanover to the British throne was extremely unpopular in Oxford, and Hearne relates how on May 28, 1715, an attempt to celebrate the King's birthday was a stormy failure, while rioting on a large scale broke out next day.

'The people run up and down, crying, *King James the Third! The True King, No usurper! The Duke of Ormond!* &c., and healths were everywhere drunk suitable to the occasion, and every one at the same time drank to a new restauration, which I heartily wish may speedily happen.' . . .

'June 5. King George being informed of the proceedings of the cavaliers at Oxford, on Saturday and Sunday (May 28, 29), he is very angry, and by his order Townshend, one of the Secretaries of State, hath sent rattling letters to Dr. Charlett, pro-vice-chancellor, and the Mayor. Dr. Charlett shewed me his this morning. This lord Townshend says his majesty (for so they will stile this silly usurper) hath been fully assured that the riots both nights were begun by scholars, and that scholars promoted them, and that he (Dr. Charlett) was so far from discountenancing them, that he did not endeavour in the least to suppress them. He likewise observed that his majesty was as well informed that the other magistrates were not less remiss on these occasions. The heads have had several meetings upon this affair, and they have drawn up a programme, (for they are obliged to do something) to prevent the like hereafter; and this morning very early, old Sherwin the yeoman beadle was sent to London to represent the truth of the matter.'

These measures had a marked effect upon the celebration on June 10 of 'King James the III'd's' birthday. Special

precautions were taken to prevent a riotous outbreak. 'So that all honest men were obliged to drink King James's health, and to shew other tokens of loyalty, very privately in their own houses or else in their own chambers, or else out of town. For my own part I walked out of town to Foxcomb, with honest Will Fullerton, and Mr. Sterling, and Mr. Eccles, all three non-juring civilians of Palliol College, and with honest Mr. John Leake, formerly of Hart Hall, and Rich. Clements (son to old Harry Clements the bookseller) he being a cavalier. We were very merry at Foxcombe, and came home between nine and ten,' &c. Several of the party were challenged on their return to Oxford, but no further mention is made of Stirling.

On August 15 there was again rioting at Oxford, in which a prominent part was taken by scholars of Balliol. There can be little doubt that Stirling was implicated, though he seems to have displayed a commendable caution on June 10 by going out of town with a man so well known as Hearne. His own account of current events is given in the following letter to his father, which is the only trace of Jacobite correspondence with Scotland that has been preserved, if it can be so termed:—

Oxon 23 July 1715.

Sir,

I wrote to you not long ago, but I have had no letter this pretty while. The Bishop of Rochester and our Master have renewed an old quarrell: the Bishop vents his wrath on my countrymen, and now is stopping the paying of our Exhibitions: it's true we ought to take Batchelours degrees by the foundation of these exhibitions, and quite them when we are of age to go into orders: Rochester stands on all those things, which his Predecessours use not to mind, and is resolved to keep every nicety to the rigor of the statute; and accordingly he hath stoped our Exhibitions for a whole year, and so owes us 20 lib. apiece. he insists on knowing our ages, degrees, and wants security for our going into orders. I suppose those things may come to nought in a little while, the Bishop is no enemy to our principles. In the meantime I've borrowed money of my friends till I'm ashamed to borrow any more. I was resolved not to trouble you while I could otherwise subsist; but now I am forced to ask about 5 lib. or what in reason you think fit to supply my present needs:

for ye little debts I have I can delay them I hope till the good humor shall take the Bishop. I doubt not to have the money one time or another, it's out of no ill will against us that he stops it, but he expects our wanting the money will make us sollicite our Master to cringe to him, which is all he wants.

No doubt you know what a generall change of the affections of the people of England the late proceedings hath occasion: the mobbs begun on the 28 of May to pull down meeting houses and whiggs houses, and to this very day they continue doing the same, the mobb in Yorkshire and Lancashire amounted to severall thousands, and would have beat of the forces sent against them had they not been diswaded by the more prudent sort, and they are now rageing in Coventry and Baintry: so (as the court saith) the nation is just ripe for a rebellion. There were severall houses of late at London searched for the Chevalier, the D. of Berwick and Mr Lesly. Oxford is impeached of high treason and high crimes and misdemeanors and is now in the Tower, a little while ago both Whiggs and Tories wished him hanged, but he has gained some tories to stand his friends in opposition to the Whiggs. They cant make out enough to impeach the rest they designed. I had a letter from Northside<sup>1</sup> lately. I shall delay an answeare till I have the occasion of a frank. My cousin James sent me a letter the other day from Amsterdam, he is just come from the Canaries, and designs to return there without coming to Britain, he remembers himself very kindly to you and all friends with you. I give my humble duty to you and my mother and my kind respects to my brothers sisters and all my relations

I am Sir

Your most dutifull son

JAS. STIRLING.

It was in the same year (1715) that Stirling first gave indications of his ability as a mathematician. In a letter<sup>2</sup> to Newton, of date Feb. 24, 1715, John Keill, of Oxford, mentions that the problem of *orthogonal trajectories*, which had been proposed by Leibniz, had recently been solved by 'Mr. Stirling, an undergraduate here', as well as by others.

The statement commonly made that Stirling was expelled

<sup>1</sup> James Stirling, son of the Laird of Northside (near Glasgow), is specially mentioned in the *List of Persons concerned in the Rebellion of 1745-6* (Scot. Hist. Soc.).

<sup>2</sup> Macclesfield, *Correspondence of Scientific Men, &c.*, vol. ii, p. 421.

from Oxford for his Jacobite leanings, and driven to take refuge in Venice, seems entirely devoid of foundation. Again Hearne's *Diary* comes to our aid, and indicates that Stirling was certainly under the observation of the government authorities:—

‘ 1715 Dec. 30 (Fri)

On Wednesday Night last Mr Sterling, a Scotchman, of Balliol Coll. and Mr Gery, Gentleman Commoner of the same College, were taken up by the Guard of the Souldiers, now at Oxford, and not released till last night. They are both honest, non-juring Gentlemen of my acquaintance.’

Also:

‘ 1716

July 21 (Sat.) One Mr Sterling, a Non-juror of Bal. Coll. (and a Scotchman), having been prosecuted for cursing K. George (as they call the Duke of Brunswick), he was tried this Assizes at Oxford, and the Jury brought him in not guilty.’

The Records of Balliol bear witness to his tenure of the Snell and Warner Exhibitions down to September, 1716. (Also as S.C.L.<sup>1</sup> of one year's standing in September, 1715, and as S.C.L. in September, 1716.) There is no indication of his expulsion, though the last mention of him by Hearne informs us that he had lost his Scholarship for refusing to take ‘the Oaths’.

‘ 1717.

March 28 (Fri)

Mr Stirling of Balliol College, one of those turned out of their Scholarships upon account of the Oaths, hath the offer of a Professorship of Mathematicks in Italy, w<sup>ch</sup> he hath accepted of, and is about going thither. This Gentleman is printing a Book in the Mathematical way at the Theatre.<sup>2</sup>

We shall see presently that Stirling found himself compelled to refuse the proffered Chair. The circumstances in which he had this offer are somewhat obscure; and whether he

<sup>1</sup> S.C.L. was a Degree (Student of Civil Law) parallel to that of B.A., just as that of Bachelor of Civil Law (B.C.L.) is parallel to that of M.A. The degree has long been abolished, but its possession would suggest that Stirling had at one time the idea of adopting the profession of his grandfather, Lord Garden.

<sup>2</sup> The Sheldonian Theatre, Oxford.

played any part in the Newton-Leibniz controversy is not certain. In the later stages of the controversy an intermediary between Leibniz and Newton was found in the Abbé Conti, a noble Venetian, born at Padua in 1677, who, after spending nine years as a priest in Venice, gave up the Church, and went to reside in Paris, where he became a favourite in society. In 1715, accompanied by Montmort, he journeyed to London, and received a friendly welcome from Newton and the Fellows of the Royal Society. In a letter<sup>1</sup> to Brook Taylor in 1721, Conti relates how 'Mr Newton me pria d'assembler à la Société les Ambassadeurs et les autres étrangers'. Conti and Nicholas Tron, the Venetian Ambassador at the English Court, became Fellows at the same time in 1715.

How Conti came to meet Stirling is unknown to us; but he must have formed a high opinion of Stirling's ability and personal accomplishments, for Newton in a letter quoted by Brewster (*Life of Newton*, ii, p. 308) querulously charges Conti with 'sending Mr. Stirling to Italy, a person then unknown to me, to be ready to defend me there, if I would have contributed to his maintenance'. The fact that Newton was a subscriber to Stirling's first venture, *Lineae Tertii Ordinis Newtonianae, sive Illustratio Tractatus D. Newtoni De Enumeratione Linearum Tertii Ordinis*, and doubtless the 'Book' mentioned by Hearne, would suggest that Newton had met Stirling before the latter had left England. This little book is dedicated to Tron, and it was on Tron's invitation that Stirling accompanied him to Italy with a view to a chair in one of the Universities of the Republic. The long list of subscribers, the majority of whom were either Fellows or Students at Oxford, bears eloquent testimony to the reputation he had acquired locally at least as a good mathematician. The book was printed at the Sheldonian Theatre, and bears the *Imprimatur*, dated April 11, 1717, of John Baron, D.D., the Vice-Chancellor of the University, and Master of his own College of Balliol, who was also subscriber for six copies. Of the subscribers, forty-five are associated with Balliol. Richard Rawlinson, of St. John's, was also a

<sup>1</sup> Printed in the posthumous *Contemplatio Philosophica* of Brook Taylor.

subscriber, and W. Clements, the bookseller in London, took six copies. Thus Stirling left Oxford after publishing a mathematical work that was to earn him a reputation abroad as a scholar.

### VENICE

From his residence in Venice,<sup>1</sup> Stirling is known in the Family History of the Stirlings as *James Stirling the Venetian*.

The invitation to Italy and the subsequent refusal are thus recorded in the Rawlinson MSS. in the Bodleian (materials collected by Dr. Richard Rawlinson for a continuation of Ward's *Athenae Oxonienses* up to 1750):

‘Jacobus Stirling, e coll. Baliol, exhibit. Scot. a Snell. jurament. R. G.<sup>2</sup> recus. 1714, et in Italianum Nobilem virum Nicolaum Tron, Venetiarum Reipublicae ergo apud Anglos Legatum, secutus est, ubi religionis causa matheseos professorium munus sibi oblatum respuit.’

The religious difficulty must have been a serious blow to Stirling's hopes, and placed him in great embarrassment, for his means were of the scantiest. But adherence to the Anglican Church was one of the most fundamental principles of the Tories, which had caused so much wavering in their ranks for the Catholic Chevalier, and there was no getting over the objection. We need not be surprised, therefore, that he got into serious difficulties, from which he was rescued in 1719 by the generosity of Newton, who had henceforward at least, Stirling for one of his most devoted friends. Stirling's

<sup>1</sup> I have endeavoured to ascertain the university to which Stirling was called. Professor G. Loria has informed me that it was very probably Padua, Padua being the only university in the Republic of Venice, the *Quartier Latin* of Venice according to Renan. It had been customary to select a foreigner for the chair of Mathematics. A foreigner (Hermann) held it, and resigned it in 1713. It was then vacant until 1716, when Nicholas Bernoulli (afterwards Professor of Law at Bâle) was appointed. Professor Favaro of Padua confirms the above, and adds that possibly some information might be gathered from the reports of the Venetian Ambassador, or from the records of the *Reformatores Studii* (the patrons of chairs in a mediaeval university). To get this information it would be necessary to visit Venice. My chief difficulty here is to reconcile the date of Stirling's visit to Italy and the date of the vacancy. It may be added that a College for Scotch and English students still flourished at Padua at this time (*see also Evelyn's Diary*). C. T.

<sup>2</sup> King George.

letter to Newton, expressing his gratitude, is here given. It has been copied from Brewster's *Life of Newton*.

Letter

Venice 17 Aug. 1719.

Sir

I had the honour of your letter about five weeks after the date. As your generosity is infinitely above my merite, so I reckon myself ever bound to serve you to the utmost: and, indeed, a present from a person of such worth is more valued by me than ten times the value from another. I humbly ask pardon for not returning my grateful acknowledgments before now. I wrote to M<sup>r</sup> Desaguliers to make my excuse while in the meantime I intended to send a supplement to the papers I sent, but now I'm willing they be printed as they are, being at present taken up with my own affair here wherewith I won't presume to trouble you having sent M<sup>r</sup> Desaguliers a full account thereof.

I beg leave to let you know that M<sup>r</sup> Nicholas Bernoulli proposed to me to enquire into the curve which defines the resistances of a pendulum when the resistance is proportional to the velocity. I enquired into some of the most easy cases, and found that the pendulum, in the lowest point had no velocity, and consequently could perform but one half oscillation, and then rest. Bernoulli had found that before, as also one Count Ricato, which I understood after I communicated to Bernoulli what occurred to me. Then he asked me how in that hypothesis of resistance a pendulum could be said to oscillate since it only fell to the lowest point of the cycloid, and then rested. So I conjecture that his uncle sets him on to see what he can pick out of your writings that may any ways be cavilled against, for he has also been very busy in enquiring into some other parts of the Principles.

I humbly beg pardon for this trouble, and pray God to prolong your daies, wishing that an opportunity should offer that I could demonstrate my gratefulness for the obligations you have been pleased to honour me with,

I am with the greatest respect Sir

Your most humble & most obedient serv<sup>t</sup>

JAMES STIRLING.

Venice 17 August 1719 n. st.

P.S. M<sup>r</sup> Nicholas Bernoulli, as he hath been accused by Dr Keill of an illwill towards you, wrote you a letter some time ago to clear himself. But having in return desired me

to assure you that what was printed in the *Acta Paris.* relating to your 10 Prop., lib. 2, was wrote before he had been in England sent to his friends as his private opinion of the matter, and afterwards published without so much as his knowledge. He is willing to make a full vindication of himself as to that affair whenever you'll please to desire it. He has laid the whole matter open to me, and if things are as he informs me Dr Keill has been somewhat harsh in his case. For my part I can witness that I never hear him mention your name without respect and honour. When he showed me the *Acta Eruditorum* where his uncle has lately wrote against Dr Keill he showed me that the theorems there about Quadratures are all corollarys from your Quadratures; and whereas Mr John Bernoulli had said there, that it did not appear by your construction of the curve, Prop. 4, lib. 2, that the said construction could be reduced to Logarithms, he presently showed me Coroll. 2 of the said Proposition, where you show how it is reduced to logarithms, and he said he wondered at his uncle's oversight. I find more modesty in him as to your affairs than could be expected from a young man, nephew to one who is now become head of Mr Leibnitz's party; and among the many conferences I've had with him I declare never to have heard a disrespectful word from him of any of our country but Dr Keill.

How long he lived in Italy after his letter to Newton is not known; but life in the cultured atmosphere of Venice must have been, otherwise, very congenial. It was a favourite haunt of the different members of the Bernoullian family. The earliest letter to Stirling of a mathematical nature that has been preserved is one in 1719 from Nich. Bernoulli, F.R.S., at that time Professor in the University of Padua. One is tempted to inquire whether Stirling did not meet Bernoulli and Goldbach on the occasion of their visit to Oxford in 1712. In the letter in question Bernoulli specially refers to their meeting in Venice, and also conveys the greetings of Poleni, Professor of Astronomy at Padua. At the same time Riccati was resident in Venice, which he refused to leave when offered a chair elsewhere. Ramsay says that Stirling made contributions to mathematics while resident in Italy, copies of which he brought home with him: but I have found no trace of them. The only paper of this period is his *Methodus Differentialis Newtoniana*, published in the *Philo-*

*sophical Transactions* for 1719, with the object of elucidating Newton's methods of Interpolation.

## LONDON

From 1719 to 1724 there is a gap in our information regarding Sterling. But a fragment of a letter by him to his brother, Mr. John Stirling of Garden, shows that in July 1724 he was at Cader (Cawder or Calder, where the family of his uncle James, the dispossessed Laird of Keir, resided). Early in 1725 he was in London, as a letter to his brother John informs us (London, 5 June, 1725) when he was making an effort towards 'getting into business'. 'It's not so easily done, all these things require patience and diligence at the beginning.' In the meantime, that he may not be 'quite idle' he is preparing for the press an edition of . . .<sup>1</sup> *Astronomy* to which he is 'adding some things'; but for half a year the money will not come in, and he hopes his mother will provide towards his subsistence.

'So I cannot go to the country this summer but I have changed my lodgings and am now in a French house and frequent french Coffeehouses in order to attain the language which is absolutely necessary. So I have given over thoughts of making a living by teaching Mathematicks, but at present I am looking out sharp for any chub I can get to support me till I can do another way. S Isaac Newton lives a little way of in the country. I go frequently to see him, and find him extremely kind and serviceable in every thing I desire but he is much failed and not able to do as he has done . . . . Direct your letters to be left at Forrest's Coffee House near Charing Cross.'

Thus in 1725, at 32 years of age, Stirling had not yet found a settled occupation which would furnish a competency. This project of 'getting into business' was given up, for, some time after, he acquired an interest in Watt's Academy in Little Tower Street, where (*Dict. Nat. Biog.*) he taught Mechanics and Experimental Philosophy. It was the same Academy in which his countryman Thomson, the poet, taught for six months from May 1726, and where the latter composed portions of 'Summer'. For about ten years Stirling was

<sup>1</sup> The name, unfortunately, is not legible.

connected with the Academy, and to this address most of the letters to him from contemporary mathematicians, that have been preserved, were directed. They form part of a larger collection that was partly destroyed by fire, and early in the nineteenth century they were nearly lost altogether through the carelessness of Wallace and Leslie of Edinburgh University, to whom they had been sent on loan from Garden. There are also a few letters to his friends in Scotland from which one can gather a certain amount of information. In the earlier days of his struggle in London he may have had to seek assistance from them, but as his circumstances improved he showed as great a generosity in return. By 1729 he could look forward with confidence to the future, for by that time he was able to wipe out his indebtedness in connection with his installation in the Academy, as the following extracts from his letters show.

In a letter to his brother, dated April 1728, he writes:

‘I had 100 Lib. to pay down here when I came first to this Academy, and now have 70 Lib. more, all this for Instruments, and besides the expenses I was at in fitting up apartments for my former project still ly over my head.’

Again on July 22, 1729, he writes:

‘Besides with what money I am to pay next Michaelmas I shall have paid about 250 Lib. since I came to this house, for my share of the Instruments, after which time I shall be in a way of saving, for I find my business brings in about 200 L. a year, and is rather increasing, and 60 or 70 L. serves me for cloaths and pocket money. I designed to have spent some time this summer among you, but on second thoughts I choose to publish some papers during my Leisure time, which have long lain by me. But I intend to execute my design is seeing you next summer if I find that my affairs will permit.’

He had always a warm side for his friends in Scotland, and his letters to them are written in a bright and cheerful style. The reference to Newton is the only one he makes regarding his friends at the Royal Society, and the ‘papers’ he speaks of publishing are almost certainly his well-known Treatise the *Methodus Differentialis* (1730), the first part of which he had drawn up some eight or nine years before (*vide* a letter to Cramer). He was admitted to the Royal Society in 1726,

a distinction that put him on an equal footing with the scientists that lived in, or frequented, London. It is most probable that his acquaintance with Maclaurin began at this time. They were both intimate friends of Newton, and fervent admirers of his genius, and both eagerly followed in his footsteps. Letters that passed between them are preserved at Garden and in Aberdeen University. The opening correspondence furnishes the best account we have of the unfortunate dispute between Maclaurin and Campbell regarding the priority of certain theorems in equations (vide *Math. Gazette*, January 1919). Maclaurin placed great reliance upon Stirling's judgment, and frequently consulted Stirling while engaged in writing his *Treatise of Fluxions*.

Their later letters are mainly concerned with their researches upon the Figure of the Earth and upon the Theory of Attraction. In 1738, Stirling, at Maclaurin's special request, joined the Edinburgh Philosophical Society, in the foundation of which Maclaurin had taken so prominent a part in 1737. Maclaurin also begged for a contribution, but if Stirling gave a paper to the Society it has not been preserved or printed.

In 1727 Gabriel Cramer, Professor of Mathematics at Geneva, received a welcome from the Royal Society on the occasion of his visit to London. He formed a warm friendship for Stirling, who was his senior by about twelve years, and several of his letters to Stirling are preserved. A copy, kept by Stirling, of a letter to Cramer furnishes interesting information regarding his own views of his *Methodus Differentialis*, and also regarding the date at which the *Supplement* to De Moivre's *Miscellanea Analytica* was printed. Stirling had sent two copies of his treatise to Cramer, one of the copies being for Nich. Bernoulli, by this time Professor of Law at Bâle. Cramer had requested to be the intermediary of the correspondence between Bernoulli and Stirling in order to have the advantage of their mathematical discussions. A few letters from Bernoulli are preserved, the last bearing the date 1733. In this letter Bernoulli pointed out several errata in the works of Stirling, and observed the omission, made by both Stirling and Newton, of a species in their enumeration of Cubic Curves. Newton gave seventy-two species, and Stirling in his little book of 1717 added four

more. But there were two additional species, one of which was noted by Nicole in 1731. Murdoch in his *Newtoni Genesis Curvarum per Umbras* (1740) mentions that Cramer had told him of Bernoulli's discovery, but without furnishing a date. Bernoulli's letter not only confirms Cramer's statement, it also gives undoubted precedence to Bernoulli over Stone's discovery of it in 1736.

From 1730 onwards Stirling's life in London must have been one of considerable comfort, as his 'affairs' became prosperous, while he was a familiar figure at the Royal Society, where his opinions carried weight. According to Ramsay he was one of the brilliant group of philosophers that gathered round Polingbroke on his return from exile. Of these Stirling most admired Berkeley. If he at all shared the opinions of the disillusioned politician then he might still be a Tory, but it was improbable that he retained any loyalty to the Jacobite cause. When the Rebellion broke out in 1745 there is no trace of Stirling being implicated, though his uncle of Cawdor was imprisoned by the government and thus kept out of mischief. His studies were now directed towards the problem of the Figure of the Earth, the discussion of which had given rise to two rival theories, (i) that of Newton, who maintained that the Earth was flatter at the Poles than at the Equator, and (ii) that of the Cassinis, who held exactly the opposite view.

In 1735 Stirling contributed a short but important note on the subject which appeared in the *Philosophical Transactions* (*vide* Todhunter's *History of the Theory of Attraction and the Figure of the Earth*).

#### RETURN TO SCOTLAND

In 1735, a great change in his circumstances was occasioned by his appointment to the Managership of the Leadhills Mines in Scotland.

A more complete change from the busy social life of London to the monotonous and dreary moorland of Leadhills can hardly be imagined. At first he did not break entirely with London, but in a year or two he found it necessary to reside permanently in Scotland, and a letter from Machin to him in 1738, would suggest that he felt the change keenly.

He was now well over forty years of age, but, nothing daunted, he set himself to the discharge of his new duties with all the energy and ability at his command.

The letters he exchanged with Maclaurin and Machin show that his interest in scientific research remained unabated, though the want of time due to the absorbing claims of his new duties is frequently brought to our notice. He appears to have discovered further important theorems regarding the Figure of the Earth, which Machin urged him to print, but he never proceeded to publication. His reputation abroad, however, led the younger school of rising mathematicians to cultivate his acquaintance by correspondence, and to this we owe a letter from Clairaut, and also a long and interesting letter from Euler. Clairaut (1713-65), who had shown a remarkable precocity for mathematics, was a member of the French Commission under Maupertuis, sent out to Lapland to investigate the length of an arc of a meridian in northern latitudes, a result of which was to establish conclusively Newton's supposition as against the Cassinians. As Voltaire put it: Maupertuis 'avait aplati la Terre et les Cassinis.' While still in Lapland Clairaut sent to the Royal Society a paper, some of the conclusions in which had been already communicated by Stirling. An apology for his ignorance of Stirling's earlier publication furnished Clairaut with the ground for seeking the acquaintance of Stirling in 1738, and requesting his criticism of a second paper on the Figure of the Earth.

The correspondence with Euler in 1736-8, in connection with the Euler-Maclaurin Theorem, has already been referred to by me in the *Math. Gazette*. Euler (1707-83) is the third member of the famous Swiss school of mathematicians with whom Stirling had correspondence. From his letters to Daniel Bernoulli (Fuss, *Corr. Math.*) it is quite clear that Euler was familiar with Stirling's earlier work.

Stirling was so much impressed by Euler's first letter that he suggested that Euler should allow his name to be put up for fellowship of the Royal Society. Euler's reply, which is fortunately preserved, is remarkable for its wonderful range of mathematical research; so much so that Stirling wrote to Maclaurin that he was 'not yet fully master of it.'

Euler, who was at the time installed in Petrograd, did not then become a Fellow of the Royal Society. In 1741 he left Russia for Berlin, where, in 1744, he was made Director of the Mathematical Section of the Berlin Academy, and it is quite possible that he had a share in conferring upon Stirling the honorary membership of the Academy in 1747. The information is contained in a letter of that date from Folkes, P.R.S., conveying the message to Stirling with the compliments of Maupertuis, the President, and the Secretary, De Formey.

The letter furnishes the last glimpse we have of Stirling's connection with London. (He resigned his membership of the Royal Society in 1754.)

### LEADHILLS

Regarding Stirling's residence in Scotland we are fortunately provided with much definite information. A detailed account of his skilful management of the mines is given in the *Gentleman's Magazine* for 1853.<sup>1</sup> He is also taken as one of the best types of the Scotsmen of his day by Ramsay in his *Scotland and Scotsmen*.

Ramsay, who always speaks of him as the *Venetian*, met him frequently on his visits to Keir and Garden, and had a profound regard for the courtly and genial society of the Venetian, who by his long residence abroad and in London had acquired to a marked degree *la grande manière*, without any trace of the pedantry one might have expected. Ramsay also narrates several anecdotes regarding Stirling's keen sense of humour.<sup>2</sup>

The association between Venice and the Leadhills in Stirling's career is very remarkable. According to Ramsay, before Stirling left Venice, he had, at the request of certain London merchants, acquired information regarding the manufacture of plate glass. Indeed, it is asserted by some that owing to his discovery he had to flee from Venice, his life being in danger, though Ramsay makes no mention of this. Be that as it may, his return to London paved the way for further acquaintance, with the result that about 1735 the Scots Mining Company, which was controlled by a group

<sup>1</sup> 'Modern History of Leadhills'.

<sup>2</sup> *l. c.*, vol. ii.

of London merchants, associated with the Sun Fire Office, selected him as manager of the Leadhills mines. The company had been formed some twenty years previously with the object of developing the mining for metals, and had for managing director Sir John Erskine of Alva, a man of good ideas, but lacking in business capacity to put them into practice. Leases were taken in different parts of the country, but were all given up, with the exception of that of the Leadhills mines, the property of the Hopetoun family, which had already been worked for over a century. When Stirling was appointed the affairs of the Company were in a bad way.

For the first year or more Stirling only resided at the mines for a few weeks, but about 1736 he took up definite residence, devoting his energies entirely to the interests of the Company. Gradually the debts that had accumulated in his predecessor's day were cleared off, and the mines became a source of profit to the shareholders. But his scientific pursuits had to be neglected. We find him, in his letters to Maclaurin, with whom he still frequently corresponded, complaining that he had no time to devote to their scientific researches, and when writing to Euler he tells him that he is so much engrossed in business that he finds difficulty in concentrating his thoughts on mathematical subjects in the little time at his disposal.

The village in which he and the miners lived was a bleak spot in bare moorland, nearly 1,300 feet above sea level. There was no road to it, and hardly even a track. Provisions and garden produce had to be sent from Edinburgh or Leith. In spite of these disadvantages Stirling has left indelible traces of his wise management, and many of his improvements have a wonderful smack of modernity. The miners were a rough, dissipated set of men, who had good wages but few of the comforts of life. Stirling's first care was to add to their comfort and to lead them by wise regulations to advance their own physical and mental welfare.

In the first place he carefully graded the men, and worked them in shifts of six hours, so that with a six hours' day they had ample time at their disposal. To turn their leisure to profit they were encouraged to take up, free of charge, what we should now call 'allotments', their size being restricted only

by the ability of the miners to cultivate. The gardens or crofts produced fair crops, and in time assumed a value in which the miner himself had a special claim, so that he could sell his right to the ground to another miner without fear of interference from the superior. In this way Stirling stimulated their industry, while at the same time furnishing them with a healthy relaxation from their underground toil. The miners were subject to a system of rules, drawn up for their guidance, by reference to which disputes could be amicably settled. They had also to make contributions for the maintenance of their sick and aged. In 1740, doubtless with the aid of Allan Ramsay, the poet, who was a native of the place, a library was instituted, to the upkeep of which each miner had to make a small subscription. Stirling is thus an early precursor of Carnegie in the foundation of the free library. When Ramsay of Ochertyre visited Leadhills in 1790 the library<sup>1</sup> contained several hundred volumes in the different departments of literature, and it still exists as a lasting memorial to Stirling's provision for the mental improvement of the miners.

On the other hand, Stirling's own requirements were well provided for by the Company, whose affairs were so prosperous under his control. They saw to it that he was well housed. More than once they stocked his cellar with wines, while the salary they paid him enabled him to amass a considerable competency. When, with the increase of years, he became too frail to move about with ease, they supplied him with a carriage.

#### THE GLASGOW KETTLE

In the eighteenth century the rapidly expanding trade of Glasgow and the enterprise of her merchants made it highly desirable to have better water communication and to make the city a Port, and in 1752 the Town Council opened a separate account to record the relative expenditure. The

<sup>1</sup> Of Stirling's private library two books have been preserved. One, on Geometry, was presented to him by Bernoulli in 1719. The other (now at Garden) is his copy of Brook Taylor's *Methodus Incrementorum*, which he bought in 1725.

first item in this account, which is headed 'Lock design'd upon the River of Clyde', runs thus:

'Paid for a compliment made by the Town to James Stirling, Mathematician for his service, pains, and trouble, in surveying the River towards deepening by locks, vizt

For a Silver Tea Kettle and Lamp weighing $66\frac{1}{4}$ oz	
at 8/ per oz	£26 10 0
For chasing & Engraving the Towns arms	1 14 4
	<hr/> £28 4 4'

Stirling had evidently performed his task gratuitously but with characteristic thoroughness; and to this day, when the city holds festival, the Kettle is brought from Garden, where it reposes, in grateful memory of the services that occasioned the gift.

To this period there belongs only one paper by Stirling, a very short article (*Phil. Trans.*, 1745) entitled 'A Description of a machine to blow Fire by the Fall of Water'. The machine is known to engineers as *Stirling's Engine*, and furnishes an ingenious mechanical contrivance to create a current of air, due to falling water, sufficiently strong to blow a forge or to supply fresh air in a mine. Its invention is doubtless due to a practical difficulty in his experience as a mining manager.

There is also preserved at Garden the manuscript of a treatise by Stirling on *Weights and Measures*.

For thirty-five years Stirling held his managership. He died in 1770, at the ripe age of seventy-eight, when on a visit to Edinburgh to obtain medical treatment. Like Maclaurin and Matthew Stewart, he was buried in Greyfriars Churchyard, 'twa' corps lengths west of Laing's Tomb',<sup>1</sup> as the Register Records grimly describe the locality.

By his marriage with Barbara Watson, daughter of Mr. Watson of Thirtyacres, near Stirling, he had a daughter, Christian, who married her cousin, Archibald Stirling of Garden, his successor as manager of the mines; and their descendants retain possession of the estate of Garden.

<sup>1</sup> Laing's Tomb is a prominent mural tablet (1620) on the right wall surrounding the churchyard.

Thus closed a career filled with early romantic adventure and brilliant academic distinction, followed in later years by as marked success in the industrial field. As a mathematician Stirling is still a living power, and in recent years there has sprung up, more particularly in Scandinavian countries, quite a Stirling cult. His is a record of successful achievement of which any family might well be proud.

# WORKS PUBLISHED BY J. STIRLING

(A)

## ENUMERATION OF CUBICS

§ 1. His first publication, *Lineae Tertii Ordinis Neutonianae sive Illustratio Tractatus D. Newtoni De Enumeratione Linearum Tertii Ordinis. Cui subjungitur, Solutio Trium Problematum*, was printed at the Sheldonian Theatre, at Oxford, in 1717.

As the book<sup>1</sup> is very scarce, I give a short account of its leading contents.

By a transcendent effort of genius, Newton had, in the publication of his *Enumeration of Cubic Curves*, in 1704, made a great advance in the theory of higher plane curves, and brought order into the classification of cubics.

He furnished no proofs of his statements in his tractate. Stirling was the first of three mathematicians from Scotland who earned for themselves a permanent reputation by their commentaries on Newton's work. Stirling proved all the theorems of Newton up to, and including, the enumeration of cubics. Maclaurin developed the organic description of curves (the basis for which is given by Newton), in his *Geometria Organica* (1720); and P. Murdoch<sup>2</sup> gave, in his *Genesis Curvarum per Umbras* (1740), a proof that all the curves of the third order can be obtained by suitable projection from one of the five divergent parabolas given by the equation

$$y^3 = ax^3 + bx^2 + cx + d.$$

Stirling, in his explanatory book, follows precisely on the lines suggested by Newton's statements, though I doubt whether he had the assistance of Newton in so doing; for

<sup>1</sup> Edleston (*Correspondence*, &c., p. 235) refers to a letter from Taylor to Keill, dated July 17, 1717, which gives a *critique* of Stirling's book.

<sup>2</sup> Earlier proofs were given by Nicole and Clairaut in 1731 (*Mém. de l'Acad. des Sciences*).

in that case why should he have stopped short with but half of the theory?

§ 2. Newton stated that the algebraic equation to a cubic can be reduced to one or other of the four forms (i)  $xy^2 + cy$ , or (ii)  $xy$ , or (iii)  $y^2$ , or (iv)  $y = ax^3 + bx^2 + cx + d$ ; and he gave sufficient information as to the circumstances in which these happen.

The demonstration of this statement forms the chief difficulty in the theory.

Stirling finds it necessary to devote two-thirds of his little book of 128 pages to introductory matter. He bases the analytical discussion on Newton's doctrine of *Series*, and gives an adequate account of the use of the Parallelogram of Newton for expanding  $y$  in ascending or descending powers of  $x$ ,  $x$  and  $y$  being connected by an algebraic equation. (He also applies his method to fluxional or differential equations, though here he is not always very clear.) With some pride he gives on p. 32 the theorem<sup>1</sup>

$$\text{Let} \quad y = A + Bx^r + Cx^{2r} + \dots,$$

then  $y$  may be expressed as

$$y = A + \frac{x\dot{y}}{1 \cdot r\dot{x}} + \frac{x^2\ddot{y}}{1 \cdot 2 \cdot r^2\dot{x}^2} + \frac{x^3\ddot{\ddot{y}}}{1 \cdot 2 \cdot 3 \cdot r^3\dot{x}^3} + \&c.$$

applicable when  $x$  is very large if  $r$  is negative, or when  $x$  is very small if  $r$  is positive. As an example he establishes the Binomial Theorem of Newton (p. 36).

Pages 41-58 are taken up with the general theory of asymptotes. A rectilinear asymptote can cut the curve of degree  $n$  in, at most,  $n-2$  finite points. If two branches of the curve touch the same end of an asymptote, or opposite ends but on the same side of the asymptote, then three points of intersection go off to infinity.

A curve cannot have more than  $n-1$  parallel asymptotes, and if it has  $n-1$ , then it cannot cut these in any finite point.

If the  $y$ -axis is parallel to an asymptote, the equation to the curve can have no term in  $y^n$ . From this follows the important corollary that the equation to a cubic curve may always be found in the form

$$(x+a)y^2 = yf_2(x) + f_3(x).$$

<sup>1</sup> Cf. Maclaurin's Theorem.

For all lines of odd degree have real points at infinity.

Asymptotes may be found by the doctrine of series: but not always.

Thus the quartic  $y = (ax^4 + bx^3 + \dots + e)/(fx^3 + gx^2 + hx + k)$  has the asymptote

$$y = \frac{ax}{f} + \frac{bf - ag}{f^2},$$

as found by a series.

The rest of the asymptotes are given by  $x = \alpha$ , where  $\alpha$  is any one of the roots of

$$fx^3 + gx^2 + hx + k = 0.$$

In the standard case of an equation of degree  $n$  in  $x$  and  $y$ ,

$$u_n + u_{n-1} + \dots + u_0 = 0,$$

if we assume the series

$$y = Ax + B + \frac{C}{x} + \frac{D}{x^2} + \dots$$

and substitute in the given equation we find, in general,

- (1) an equation of degree  $n$  for  $A$  furnishing  $n$  values of  $A$ ,
- (2) an equation involving  $A$  and  $B$  of the first degree in  $B$ ,
- (3) an equation in  $A$ ,  $B$ , and  $C$ , of the first degree in  $C$ , &c.

So that in general we may expect  $n$  linear asymptotes

$$y = Ax + B.$$

§ 3. Pages 58-69, with the diagrams, furnish quite a good introduction to what we now call *graph-tracing*.

He thus graphs the rational function  $y = f(x)/\phi(x)$  with its asymptotes parallel to the  $y$ -axis found by equating  $\phi(x)$  to zero.

The manner in which a curve approaches its asymptotes is explained by means of series.

In the curves given by  $y = a + bx + \dots + kx^n$  there are only two infinite branches which are on the same, or opposite, sides of the  $x$ -axis, according as  $n$  is even or odd. When  $x$  is large the lower terms in  $x$  may be neglected as compared with  $kx^n$ .

Then follows the graphical discussion of quadratic, cubic, and quartic equations in  $x$ . The graph of  $y = x^2 + ax + b$  shows that the roots of the corresponding quadratic equation

in  $x$  are real or imaginary according as the turning value of  $y$  is negative or positive.

For the cubic  $x^3 + ax^2 + bx + c = 0$  he gives the excellent rule, which has recently been resuscitated, that the three roots are real and distinct only when the graph of the corresponding function has two real turning values opposite in sign. A similar test is applied to discuss the reality of the roots of a quartic. These results are required later in the enumeration of cubic curves.

On p. 69 he gives the important theorem that a curve of degree  $n$  is determined by  $\frac{1}{2}n(n+3)$  points on it.<sup>1</sup>

The demonstrations of Newton's general theorems in higher plane curves are then given in detail.

An indication of some of these is interesting, and the modern geometer will note the entire absence of trigonometry.

#### § 4. Diameter Theorem.

Draw a line in a given direction to cut a curve in  $P_1, P_2 \dots P_n$ ; and find  $O$  on it such that  $\Sigma OP = 0$ .

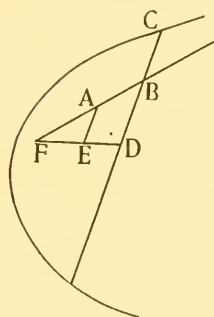


FIG. 1.

As the line varies in position  $O$  generates a straight line.

Let the equation to the curve be

$$y^n + (ax + b)y^{n-1} + \dots = 0. \quad (1)$$

In the figure let  $AB = x$ ,  $BC = y$  (so that  $A$  is what we call the *origin*).

Take  $AF = -b/a$ ; and  $AE$  parallel to  $BC$ , and equal to  $-b/n$ . Let  $ED = z$ ,  $DC = v$ ; also let  $AB/ED = \alpha$ .

Then  $x = \alpha z$ ,

$$y = DC - DB = v - \frac{a\alpha z + b}{n},$$

and substitution of these values in  $D$  leads to an equation

$$v^n + v^{n-2}f_2(z) \text{ \&c.} = 0,$$

in which the term in  $v^{n-1}$  is wanting. Let  $D$  coincide with  $O$  and  $DC$  with  $OP$ .  $\therefore$  &c. Q. E. D.

Stirling adds the extensions, not given by Newton, to a Diametral Conic, a Diametral Cubic, &c., corresponding to  $O$  when  $\Sigma OP_1 \cdot OP_2 = 0$ ,  $\Sigma OP_1 \cdot OP_2 \cdot OP_3 = 0$ , &c.

<sup>1</sup> Also stated by Hermann (*Phoronamia*).

*Newton's Rectangle Theorem for a Conic, and generalization.*

The proof is made to depend on the theorem that if  $\alpha_1, \alpha_2, \dots, \alpha_n$  are the roots of

$$\phi(x) \equiv x^n + ax^{n-1} + \dots + k = 0,$$

then  $\phi(\xi) = (\xi - \alpha_1)(\xi - \alpha_2) \dots (\xi - \alpha_n)$ .

In the case of the cubic

$$y^3 + y^2(ax + b) + y(cx^2 + dx + e) + fx^3 + gx^2 + hx + k = 0.$$

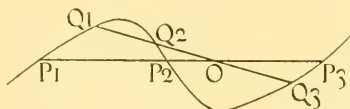


FIG. 2.

Let  $P_1OP_3$ ,  $Q_1OQ_3$  be drawn in fixed directions through a point  $O$ . Let  $P_1P_3$  be the  $x$ -axis,  $Q_1Q_3$  parallel to the  $y$ -axis, and let  $O$  be the point  $(\xi, 0)$ .

Then  $OQ_1 \cdot OQ_2 \cdot OQ_3 = f\xi^3 + g\xi^2 + h\xi + k$ ,

$$OP_1 \cdot OP_2 \cdot OP_3 = \frac{1}{f}(f\xi^3 + g\xi^2 + h\xi + k),$$

so that the quotient

$$OQ_1 \cdot OQ_2 \cdot OQ_3 / OP_1 \cdot OP_2 \cdot OP_3 = f(\text{up to sign}).$$

But a change to parallel axes does not change  $f$ .  $\therefore$  &c.

§ 5. After a brief enumeration of conics he proceeds to find in Prop. XV (p. 83) the reduction of the equation of a cubic to one or other of the four forms given by Newton.

The equation

$$(z + a)v^2 = (bz^2 + cz + d)v + ez^3 + fz^2 + gz + h \quad (1)$$

includes all lines of the third order, the  $v$ -axis being parallel to an asymptote.

First Case. Let all the terms be present in (1).

Let  $A$  be the origin,  $AB$  any abscissa  $z$ ,  $BC$  or  $BD$  the corresponding ordinate  $v$  of the cubic. If  $F$  is the middle point of  $CD$

$$BF = \frac{1}{2}(v_1 + v_2) = \frac{bz^2 + cz + d}{2z + 2a},$$

so that the locus of  $F$  is the conic

$$v = (bz^2 + cz + d) / 2z + a$$

with real asymptotes  $GE$  and  $GH$ .

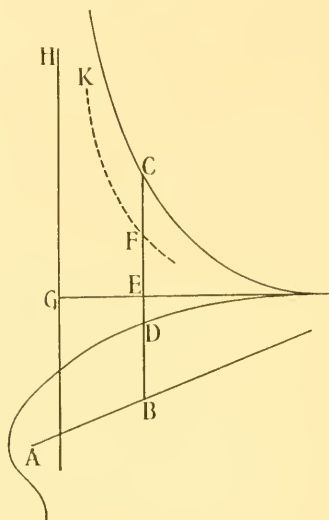


FIG. 3.

Select these lines as new axes.

Call  $GE$   $x$ , and  $EC$  or  $ED$   $y$ .

The cubic equation is of the same form as before, but  $EF$  must  $= K/2x$ , where  $K$  is constant, by the nature of the hyperbola. Therefore, the equation to the cubic is of the form

$$y^2 - ey/x = ax^2 + bx + c + d/x,$$

$$\text{or} \quad xy^2 - ey = ax^3 + bx^2 + cx + d. \quad (\text{I})$$

With a good deal of ingenuity, the proof is indicated in the other cases.

Prop. XVI (p. 87).

When  $a$  is positive in (I) all three asymptotes are real. They are

- (i)  $x = 0$ ,
- (ii)  $y = x\sqrt{a} + b/2\sqrt{a}$ ,
- (iii)  $y = -x\sqrt{a} - b/2\sqrt{a}$ .

If  $b = 0$ , the asymptotes are concurrent.

If  $b \neq 0$ , they form a triangle, inside which any oval of the cubic must lie, if there is an oval. The asymptotes (ii) and (iii) cut on the  $x$ -axis, which is also a median of the asymptotic triangle. When  $e = 0$ , the point at infinity on the asymptote (i) is a point of inflexion, and conversely: in that case the locus of  $F$  reduces to a straight line, which is a 'diameter' of the curve. An inflexion at infinity and a diameter are always thus associated. The condition that (ii) or (iii) cuts the curve only at infinity is  $b^2 - 4ac = \pm 4ae\sqrt{a}$ .

Thus possible conditions for a diameter are

$$e = 0.$$

$$b^2 - 4ac = 4ae\sqrt{a}.$$

$$b^2 - 4ac = -4ae\sqrt{a}.$$

When any two of these are satisfied so is the third ( $a$  is positive and not zero). Thus a cubic may have no diameter, or one diameter, or three diameters. It cannot have two.

§ 6. The enumeration of cubics is then proceeded with in the order given by Newton, to whose work the reader must go for the figures, which are not given by Stirling. Newton gave 72 species. To these Stirling added 4 species, viz. species 11, p. 99, species 15, p. 100, and on p. 102, species 24 and 25. There still remained two species to be added (both arising from the standard form  $xy^2 = ax^2 + bx + c$ ). One of them was given by Nicole in 1731, and the other was communicated by N. Bernoulli,<sup>1</sup> in a letter to Stirling in 1733.

While sufficiently lucid, Stirling's reasoning is admirably concise. He was never addicted to excess in the use of words, and often drove home the truth of a proposition by a well-chosen example, especially in his later work.

The publication of his commentary on Newton's *Cubics* gave Stirling a place among mathematicians, and may have been the ground on which he was invited by Tron to accept a chair in Venetian territory.<sup>2</sup>

<sup>1</sup> See note to Letter.

<sup>2</sup> In connection with both Newton and Stirling see W. W. Rouse Ball on 'Newton's Classification of Cubic Curves', *London Math. Soc.*, 1891. Another edition of Stirling's *Lineae Tertii Ordinis* was published in Paris in 1787. ('Isaaci Newtoni Enumeratio Linearum Tertii Ordinis. Sequitur illustratio eiusdem tractatus Iacobo Stirling.')

(B)

METHODUS DIFFERENTIALIS, SIVE TRACTATUS  
DE SUMMATIONE ET INTERPOLATIONE  
SERIERUM INFINITARUM

§ 7. The *Methodus Differentialis*, as we shall call it, is the most important product of Stirling's genius, by which he is most generally known to mathematicians. The book is not, as the title may suggest, a treatise on the Differential Calculus, but is concerned with the Calculus of Finite Differences. It is divided into: (1) the *Introduction* (pp. 1-13); (2) the *Summation of Series* (pp. 14-81); (3) the *Interpolation of Series* (pp. 85-153).

In the Introduction he explains how the Series are defined. Denote the terms by  $T, T', T'', \&c.$ , and write

$$S = T + T' + T'' + \&c.$$

Suppose the terms arranged as ordinates to a line so that consecutive terms are always at the distance unity.

Thus if  $T$  is at distance  $z$  from the origin,  $T'$  is at a distance  $z+1$ ,  $T''$  at distance  $z+2$ ,  $\&c.$ ; where  $z$  is not necessarily an integer.

For example, in Brouncker's *Series* (p. 26)

$$\frac{1}{1 \cdot 2} + \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 6} + \dots$$

any term is given by  $1/4z(z+\frac{1}{2})$  where  $z$  is, in succession,  $\frac{1}{2}, 1\frac{1}{2}, 2\frac{1}{2}, \&c.$

A series may sometimes be specified by a relation connecting terms;

e.g. if 
$$T'' = \frac{z+n}{z} T,$$

then 
$$T''' = \frac{z+n+1}{z+1} T'', \&c.$$

Theorems of special interest arise when  $T$  can be expressed as

$$T = A + Bz + Cz(z-1) + Dz(z-1)(z-2) + \&c.,$$

or as

$$T = A + \frac{B}{z} + \frac{C}{z(z+1)} + \&c.,$$

the latter being useful when  $z$  is a large number.

When  $T$  admits of either representation then after any transformation it should be reduced again to the same form.

$$\text{Thus if } T = A + Bz + Cz(z-1) + \dots,$$

$$\begin{aligned} \text{then } Tz &= (A+B)z + (B+2C)z(z-1) \\ &\quad + (C+3D)z(z-1)(z-2) + \dots \end{aligned}$$

To facilitate the reduction Stirling gives two formulae and two numerical tables.

Let

$$x(x+1)(x+2) \dots (x+n-1) = C_n^0 x^n + C_n^1 x^{n-1} + \dots + C_n^{n-1} x$$

$$\text{and } 1/x(x+1) \dots (x+n-1) = \sum_{s=0}^{\infty} (-1)^s \Gamma_n^s / x^{n+s},$$

then

$$z^n = \Gamma_2^{n-1} z + \Gamma_3^{n-2} z(z-1) + \dots + \Gamma_{n+1}^0 z(z-1) \dots (z-n+1)$$

$$\text{and } \frac{1}{z^n} = \sum_{r=n-1}^{\infty} C_r^{r-n+1} / z(z+1) \dots (z+r).$$

The first table (p. 8) furnishes the values of  $\Gamma_n^s$  for the lower values of  $n$  and  $s$ , and the second table (p. 11) the lower values of  $C_n^r$ .

Owing to the importance of these results, and the applications which Stirling makes of them, it has been proposed by Professor Nielsen<sup>1</sup> to call the numbers  $C_n^r$  the *Stirling Numbers of the First Species*, and the numbers  $\Gamma_n^s$  the *Stirling Numbers of the Second Species*.

Nielsen has discussed their properties and indicated their affinities with the Bernoullian numbers.

As an illustration Stirling deduces

$$\frac{1}{z^2 + nz} = \frac{1}{z(z+1)} + \frac{1-n}{z(z+1)(z+2)} + \dots,$$

<sup>1</sup> Nielsen, *Ann. di Mat.*, 1904; or *Theorie der Gammafunktion* (Teubner, 1906). An account in English is given by me in the *Proc. Edin. Math. Soc.*, 1918-19. Lagrange used them in his proof of Fermat's Theorem.

which is equivalent to

$$\frac{1}{x-a} = \frac{1}{x} + \frac{a}{x(x+1)} + \frac{a(a+1)}{x(x+1)(x+2)} + \dots,$$

when it is usually spoken of as *Stirling's Series*; but it had already been given before Stirling by Nicole and by Montmort.

## PARS I

### SUMMATIO SERIERUM.

§ 8. Stirling explains that he is not so much concerned with Series, the law of summation for which is obvious or well known, as with the transformation of slowly converging series into series that more rapidly converge, with their sum to any desired degree of accuracy.

$$\begin{aligned}\text{Let} \quad S &= T + T' + T'' + \dots \text{ad } \infty, \\ S' &= \quad T' + T'' + \dots \text{ad } \infty, \\ S'' &= \quad \quad T'' + \dots \text{ad } \infty, \text{ \&c.}\end{aligned}$$

Any difference-equation connecting  $S, S', \dots, T, T', \dots, z$ , may be transformed into another by writing for these, respectively

$$S', S'', \dots, T', T'', \dots, z+1.$$

But when the number of terms in the series is finite, he takes  $T$  to be the last

$$(S = \dots + T''' + T'' + T),$$

so that  $S' = S - T$ , and if  $S$  corresponds to  $z$ ,  $S'$  corresponds to  $z-1$ .

On p. 16, he quotes a theorem of Newton,<sup>1</sup> which furnishes a key to several of the theorems that follow later in the *Methodus Differentialis*.

In modern garb it may be thus stated,

$$\int_0^z z^{p-1} (1-z)^{q-1} dz = \frac{z^p (1-z)^q}{p} F(p+q, 1, p+1, z),$$

where  $F(a, b, c, z)$  denotes the hypergeometric series

$$1 + \frac{a \cdot b}{1 \cdot c} z + \frac{a(a+1) b(b+1)}{1 \cdot 2 \cdot c(c+1)} z^2 + \dots$$

<sup>1</sup> See also p. 113 of *Methodus Differentialis*.

When  $z = 1$  we have, of course, the Beta Function

$$\int_0^1 z^{p-1} (1-z)^{q-1} dz.$$

Prop. I.

$$\S 9. \text{ If } T = A + Bz + Cz(z-1) + \dots$$

the sum of the first  $z$  terms is

$$Az + \frac{B}{2}(z+1)z + \frac{C}{3}(z+1)z(z-1) + \dots,$$

and Prop. II.

$$\text{If } T = \frac{A}{z(z+1)} + \frac{B}{z \cdot z+1 \cdot z+2} + \dots,$$

$$\text{and } S = T + T' + \dots \text{ ad } \infty,$$

$$\text{then } S = \frac{A}{z} + \frac{B}{2 \cdot z \cdot z+1} + \frac{C}{3 \cdot z \cdot z+1 \cdot z+2} +, \&c.,$$

were both given previously by Nicole and Montmort, but Stirling carries their applications much further.

E.g. To sum

$$\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \dots$$

This Stirling effects in the following characteristic fashion (pp. 28, 29).

$$T = \frac{1}{z^2} = \frac{1}{z \cdot z+1} + \frac{1!}{z \cdot z+1 \cdot z+2} + \frac{2!}{z \cdot \dots \cdot z+3} + \frac{3!}{\&c.}, \&c.$$

Hence

$$S = \frac{1}{z} + \frac{1!}{2 \cdot z \cdot z+1} + \frac{2!}{3 \cdot z \cdot z+1 \cdot z+2} +, \&c.$$

Calculate  $S$  for  $z = 13$ .

$$\therefore \frac{1}{169} + \frac{1}{196} + \dots = .079,957,427.$$

$$\text{Add thereto } \frac{1}{1} + \frac{1}{4} + \dots + \frac{1}{144} = 1.564,976,638.$$

The total is 1.644,934,065.

Stirling did not probably know that this is equivalent to  $\frac{1}{6}\pi^2$ , until Euler sent him his well-known formulae for series of the kind.

Prop. III.

$$\text{If} \quad T = x^{z+n} \left\{ \frac{a}{z} + \frac{b}{z \cdot z+1} + \dots \right\},$$

then the sum (to infinity) is

$$x^{z+n} \left\{ \frac{a}{(1-x)z} + \frac{b-Ax}{(1-x)z \cdot z+1} + \frac{C-2Bx}{(1-x)z \cdot z+1 \cdot z+2} + \&c. \right\},$$

where  $A, B, C, \dots$  denote the coefficients of the terms preceding those in which they occur. Thus

$$A = \frac{a}{1-x}, \quad B = \frac{b-Ax}{(1-x)}, \quad \&c.$$

His well-chosen example gives the summation of the Series of Leibniz

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \dots \text{ad } \infty.$$

Here  $T = (-1)^{z-\frac{1}{2}} \frac{1}{z}$  as found by writing  $\frac{1}{2}, 1\frac{1}{2}, 2\frac{1}{2}, \&c.$  for  $z$ , so that  $b = 0, \&c.$  Calculate the sum for  $z = 12\frac{1}{2}$  from the formula. It is .020,797,471,9. Add thereto

$$1 - \frac{1}{3} + \dots - \frac{1}{23} = .764,600,691.5,$$

so that the sum of the total series is .785,398,163,4, a result which could never be attained by the simple addition of terms, 'id quod olim multum desiderabat *Leibnitius*'.

(Stirling sums the same series by another process on p. 66.)

This is an example of several numerical series, well known in his day, the summation of which had hitherto proved refractory, and which Stirling can sum to any desired degree of accuracy.

Prop. IV is concerned with the problem of proceeding from an equation in  $S$  and  $S'$ , say, to an equation in  $T$  and  $T'$ .

$$\begin{array}{ll} \text{E.g. From} & (z-n)S = (z-1)S', \\ \text{he finds} & (z-n)T = zT'. \end{array}$$

Prop. V is taken up with applications of IV.

§ 9. Prop. VI gives an interesting theorem (pp. 37-8).

If the equation connecting  $S$  and  $S'$  is

$$S(z^\theta + az^{\theta-1} + \dots) = mS'(z^\theta + kz^{\theta-1} + \dots),$$

then the last of the sums will be finite both ways only when  $m = 1$  and  $k = a$ .

In other words the infinite product

$$\prod_{n=1}^{\infty} \frac{1 + \frac{a}{n} + \dots}{e + \frac{f}{n} + \dots}$$

is finite both ways only when  $e = 1$  and  $a = f$ .

This is one of the earliest general tests for the convergence of an Infinite Product of which Wallis ('Wallisius noster' as Stirling calls him in his earlier book) furnished an illustration, with rigorous proof, in the formula

$$\frac{\pi}{2} = \frac{2 \cdot 2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \dots}{1 \cdot 3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \dots},$$

published in his *Arithmetica Infinitorum* in 1655.

Prop. VII gives a remarkable transformation of a series, in the discussion of which he has occasion to solve a Difference Equation by the method so universally employed nowadays of representing the solution by an Inverse Factorial Series. As stated by Stirling it runs thus:

If the equation to a series is

$$(z - n)T + (m - 1)zT' (= 0),$$

$$\text{then } S = \frac{m-1}{m} T + \frac{n}{z} \frac{A}{m} + \frac{n+1}{z+1} \frac{B}{m} + \frac{n+2}{z+2} \frac{C}{m} + \&c.$$

$$(A \text{ is } \frac{m-1}{m} T, \quad B \text{ is } \frac{n}{z} \frac{A}{m}, \&c.).$$

If we take  $T = 1$  it becomes

$$F\left(z - n, 1, z, \frac{1}{1-m}\right) = \frac{m-1}{m} F\left(n, 1, z, \frac{1}{m}\right).$$

$$\left[ \text{or } F\left(\alpha, 1, \gamma, x\right) = \frac{1}{1-x} F\left(\gamma - \alpha, 1, \gamma, \frac{x}{x-1}\right) \right]$$

As Professor Whittaker has pointed out to me, the theorem in the latter form furnishes a remarkable anticipation of the well-known transformations of the Hypergeometric Series given by Kummer (*Crelle*, 15, 1836).

In Props. VIII to XII<sup>1</sup> Stirling returns again and again to the summation or transformation of the series defined by

$$T' = \frac{z-m}{z} \cdot \frac{z-n}{z-n+1} T.$$

Professor Whittaker suggests that the relative theorems were doubtless invented to discuss the series

$$\frac{1}{z-n} + \frac{z-m}{z} \frac{1}{z-n+1} + \frac{(z-m)(z-m+1)}{z \cdot z+1} \frac{1}{z-n+2} + \&c.$$

which (up to a factor) represents the remainder after  $z-1$  terms in the series

$$\begin{aligned} & \frac{1}{1-n} + \frac{1-m}{1} \frac{1}{2-n} + \frac{1 \cdot m \cdot 2-m}{1 \cdot 2} \frac{1}{3-n} + \dots, \\ &= \int_0^1 x^{-n} (1-x)^{m-1} dx. \end{aligned}$$

After the work of Euler this integral was calculated by Gamma Functions.

§ 10. A number of theorems follow for summing a series ‘accurate vel quam proxime’, all illustrated by well-chosen examples. Then, to show that his methods apply to series already well known, he takes up their application to the summation of Recurring Series, the invention of his friend De Moivre, the Huguenot refugee, who lived and died in London. He gives extensions to series when the terms at infinity are approximately of the recurrent type.

Several examples are given of more complicated series such as  $\sum a_n x^n$  when

$$\lambda_n a_n + \lambda_{n-1} a_{n-1} + \dots = 0,$$

where  $\lambda_n, \lambda_{n-1}, \dots$  are integral functions of  $n$  of degree  $r$ , and for which he finds a differential equation (*fluxional* he calls it) of the  $r$ th order.

He would have been clearer had he adopted the representation of integral functions as given by himself in the *Introduction*.

<sup>1</sup> Cf. Andoyer, *Bull. Soc. Math. de France*, 1905.

E.g. Suppose  $r = 2$ , and write the equation in the coefficients as

$$a_n(\alpha + \beta n + \gamma n \cdot n - 1) + a_{n-1}(a + b \cdot n - 1 + c \cdot n - 1 \cdot n - 2) \\ + \&c. = 0.$$

$$\begin{aligned} \text{Let} \quad y &= \Sigma a_n x^n, \\ \therefore \dot{y} &= \Sigma n a_n x^{n-1}, \\ \ddot{y} &= \Sigma n(n-1) a_n x^{n-2}, \\ &\&c. \end{aligned}$$

$$\begin{aligned} \text{Hence} \quad &(\alpha y + \beta x \dot{y} + \gamma x^2 \ddot{y}), \\ &+ x(a y + b x \dot{y} + c x^2 \ddot{y}) \\ &+ \&c. \\ &= 0, \end{aligned}$$

or differs from zero by a function of  $x$  depending on the initial terms of the series, and easily calculated.

The differential equation being obtained, its solution has next to be found when possible, and this he proceeds to do (pp. 79-84) by means of power series. Unfortunately, in the examples he takes he is not quite accurate in his conclusions.

In the last letter from N. Bernoulli referred to above (1733) the latter remarks:

‘Sic quoque observavi te non satis rem examinasse, quando pag. 83 dicis, aequationem  $r^2 \dot{y}^2 = r^2 \dot{x}^2 - \dot{x}^2 y^2$  nulla alia radice explicabilem esse praeter duas exhibitas

$$y = x - x^3 / 6 r^2 + x^5 / 120 r^4 + \dots$$

$$y = A \times 1 - x^2 / 2 r^2 + x^4 / 24 r^4 + \dots$$

quarum prior dat sinum, et posterior cosinum ex dato arcu  $x$ , et de qua posteriore dicis, quantitatem  $A$  quae aequalis est radio  $r$  ex aequatione fluxionali non determinari. Ego non solum inveni seriem non posse habere hanc formam

$$A + Bx^2 + Cx^4 + \dots$$

nisi fiat  $A = r$ , sed utramque a te exhibitam seriem comprehendendi sub alia generali, quae haec est:

$$y = A + Bx + Cx^2 + \dots$$

in qua coefficientes  $A, B, C$ , &c. hanc sequuntur relationem

$$BB = \frac{rr - AA}{rr}, \quad C = \frac{-A}{1 \cdot 2 \cdot rr}, \quad D = \frac{-B}{2 \cdot 3 \cdot rr},$$

$$E = -\frac{C}{3 \cdot 4 \cdot rr}, \quad F = -\frac{D}{4 \cdot 5 \cdot rr}, \quad \&c.$$

Si fiat  $A = 0$ , habetur series pro sinu, sin autem  $A$  fiat  $= r$  habetur series pro cosinu; sin vero  $A$  alium habeat valorem praeter hos duos, etiam alia series praeter duas exhibitae erit radix aequationis. Similiter series illae quatuor, quae exhibes pag. 84 pro radice aequationis

$$\ddot{y} + a^2 y - x\dot{y} - x^2 \ddot{y} = 0$$

sub aliis duabus generalioribus quae ex tuis particularibus compositae sunt comprehenduntur.' Bernoulli adds his solutions. (Vide Letter in question.)

## PARS SECUNDA

### DE INTERPOLATIONE SERIERUM

§ 11. The second part contains the solution of a number of problems in the treatment of which Stirling shows remarkable analytical skill. Again and again he solves Difference Equations by his method of Inverse Factorials. This is the method now adopted by modern writers<sup>1</sup> when large values of the variables are in question. In this short sketch I can only indicate very briefly a selection of some of his conclusions.

A common principle applied is contained in the following:

Being given a series of equidistant primary terms, and the law of their formation, intermediate terms follow the same law.

Take for example the series

$$1 + 1 + 2! + 3! + 4! + \&c.$$

in which the law is  $T'_{z+1} = z T'_z$  (the law for the Gamma Function). If  $a$  is the term intermediate between 1 and 1, the corresponding intermediate terms are

$$\frac{3}{2}a, \quad \frac{5}{2} \cdot \frac{3}{2}a, \quad \frac{7}{2} \cdot \frac{5}{2} \cdot \frac{3}{2}a, \quad \&c.$$

or, as Stirling puts it,

$$b = \frac{3}{2}a, \quad c = \frac{5}{2}b, \quad \&c. \quad (\text{Page 87})$$

<sup>1</sup> Cf. Wallenberg and Guldberg, *Theorie der linearen Differenzengleichungen* (Teubner, 1911).

Prop. XVII. Every series admits of interpolation whose terms consist of factors admitting of interpolation.

Thus, given the series

$$1, \frac{r}{p} A, \frac{r+1}{p+1} B, \frac{r+2}{p+2} C, \text{ \&c.}$$

it will be sufficient to interpolate in

$$1 \quad r \quad r \cdot r + 1 \dots,$$

$$1 \quad p \quad p \cdot p + 1 \dots,$$

and divide.

§ 12. Prop. XVIII is of fundamental importance in many of the series discussed.

In the two series

$$A, \frac{r}{p} A, \frac{r+1}{p+1} B, \dots,$$

$$a, \frac{r}{q} a, \frac{r+1}{q+1} b, \dots,$$

if  $A$  and  $a$  are equal, then the term of the first series at the distance  $q-r$  from the beginning is equal to the term of the second series at the distance  $p-r$  from the beginning.

The illustrations he gives can hardly furnish a proof, for  $p-r$  and  $q-r$  are not necessarily either integral or positive. (The proof may be put in a couple of lines by the use of Gamma Functions.)

Example. Consider the series

$$1, \frac{2}{1} A, \frac{4}{3} B, \frac{6}{5} C, \dots,$$

which to meet the conditions must be written as

$$1, \frac{1}{\frac{1}{2}} A, \frac{2}{\frac{1}{2}+1} B, \frac{3}{\frac{1}{2}+2} C, \dots$$

Suppose the term at distance  $m$  wanted.

Here  $p-r = -\frac{1}{2}$ . Write  $q-r = m$  or  $q = m+1$ , and form the series

$$1, \frac{a}{m+1}, \frac{2b}{m+2}, \frac{3c}{m+3}, \dots$$

Then the term wanted in the first series is that of this second series which precedes 1 by the interval  $-\frac{1}{2}$ . This

artifice is often useful when  $m$  is a large number, provided the second series can be easily interpolated.

He leaves these considerations to lay down the standard formulae of interpolation already established by Newton, viz. that known as Newton's Interpolation Formula

$$f(z) = f(0) + A_1 z + A_2 \frac{z(z-1)}{2!}, \text{ \&c.,}$$

and also the two formulae known as Stirling's Formulae, though they are really due to Newton.

He also takes the opportunity to establish (p. 102) what is called Maclaurin's Series. 'Et hoc primusprehendit D. Taylor in *Methodo Incrementorum*, et postea Hermannus in Appendice ad *Phoronomium*.'

§ 13. In Prop. XXI he teaches by examples how to interpolate near the beginning of a series. The second example (pp. 110-12) furnishes by pure calculation a most remarkable result, represented in modern notation by the formula

$$\Gamma\left(\frac{1}{2}\right) = \sqrt{\pi}.$$

About the same time Euler had obtained the same result by a different method (vide Fuss, *Corresp. mathématique*).

Stirling proposes to find the term midway between 1 and 1 in the series

$$1, 1, 2, 6, 24, 120, \text{ \&c.}$$

The law here is  $T_{z+1} = z T_z$  and  $T_1 = 1, T_2 = 1$ .

He interpolates between  $T_{11}$  and  $T_{12}$  to find  $T_{11\frac{1}{2}}$  and then he has to divide by  $10\frac{1}{2}, 9\frac{1}{2}, \dots 1\frac{1}{2}$  to obtain  $T_{\frac{1}{2}}$ . Since the numbers are rapidly increasing he uses their logarithms instead and actually calculates  $\log T_{11\frac{1}{2}}$  from which he finds  $T_{11\frac{1}{2}}$  to be 11899423.08, so that  $T_{1\frac{1}{2}} = .8862269251$ .

He adds  $T_{\frac{1}{2}} = 1.7724538502$ , and this number, he says, is  $\sqrt{\pi}$ . ( $\sqrt{\pi}$  is actually 1.7724533509.)

Also the corresponding entry among the numbers 1, 1, 4, 36 576, \&c. is  $\pi$ .<sup>1</sup>

For inventive audacity Stirling's conclusion would be difficult to match, and its skillful application led him to

<sup>1</sup> Is it not possible that he thus detected that  $T_{\frac{1}{2}} = \sqrt{\pi}$ ?

results that aroused the admiration of his friend De Moivre. (Vide *Miscellanea Analytica*.)

In Prop. XXII, Ex. 1, it helps him in the interpolation of the term at infinity in the series

$$1, \quad \frac{2}{1}A, \quad \frac{4}{3}B, \quad \frac{6}{5}C, \dots,$$

or 
$$1, \quad \frac{2}{1}, \quad \frac{2 \cdot 4}{1 \cdot 8}, \quad \frac{2 \cdot 4 \cdot 6}{1 \cdot 3 \cdot 5}, \quad \&c.$$

a problem which faces him again in Prop. XXIII, in which he gives a formula to find the ratio of the coefficient of the middle term in  $(1+x)^{2n}$  to  $2^{2n}$ .

Binet in his Memoir<sup>1</sup> (pp. 319-20) proved that of the four solutions of the latter problem given by Stirling (1) and (3) are correct, while (2) and (4) are wrong. As a matter of fact Stirling only proves (1) and (3) and leaves (2) and (4) to the reader.

Binet, writing  $b$  for the middle coefficient, gives the four formulae

$$(1) \quad \left(\frac{2^{2n}}{b}\right)^2 = \pi n F\left(\frac{1}{2}, \frac{1}{2}, n+1, 1\right).$$

$$(2) \quad \left(\frac{2^{2n}}{b}\right)^2 = \frac{\pi}{2} (2n+1) F\left(\frac{1}{2}, -\frac{1}{2}, n+1, 1\right).$$

$$(3) \quad \left(\frac{b}{2^{2n}}\right)^2 = \frac{2}{\pi (2n+1)} F\left(\frac{1}{2}, \frac{1}{2}, n+\frac{3}{2}, 1\right).$$

$$(4) \quad \left(\frac{b}{2^{2n}}\right)^2 = \frac{1}{\pi n} F\left(\frac{1}{2}, -\frac{1}{2}, n+\frac{1}{2}, 1\right).$$

Of these (1) and (3)<sup>2</sup> are also the first and third of Stirling's; while (2) and (4) replace the other two given by Stirling, viz :

$$(2)' \quad \left(\frac{2^{2n}}{b}\right)^2 =$$

$$\frac{\pi}{2} (2n+1) \left[ 1 - \frac{1^2}{2(2n-3)} + \frac{1^2 \cdot 3^2}{2 \cdot 4(2n-3)(2n-5)}, \quad \&c. \right].$$

<sup>1</sup> Binet, *Mém. sur les Intégrales définies Eulériennes*.

<sup>2</sup> These are also the solutions he gave in a letter to De Moivre to publish in the *Miscellanea Analytica*. (See pp. 46-48.)

$$(4)' \quad \left(\frac{b}{2^{2n}}\right)^2 = \frac{1}{\pi n} \left[ 1 - \frac{1^2}{2(2n-2)} + \frac{1^2 \cdot 3^2}{2 \cdot 4(2n-2)(2n-4)}, \text{ \&c.} \right].$$

Clearly (4)' must be wrong since the factors  $2n-2, 2n-4, \dots$  include zero in their number.

Binet remarks that the products of (1) and (4) and of (2) and (3) furnish the first examples known of Gauss's law

$$F(\alpha, \beta, \gamma, 1) \times F(-\alpha, \beta, \gamma - \alpha, 1) = 1.$$

§ 14. In Prop. XXIV the Beta Function is introduced (as an Integral) for the interpolation of

$$a, \quad \frac{r}{p} a, \quad \frac{r(r+1)}{p(p+1)} a, \quad \dots$$

and the conclusion drawn (in modern notation)

$$\begin{aligned} B(p+n, q)/B(p, q) \\ = p(p+1)\dots(p+n-1)/(p+q)(p+q+1)\dots(p+q+n-1). \end{aligned}$$

Again, on p. 139, he solves the associated difference equation

$$T' = \frac{r+z+n}{r+z} T,$$

obtaining  $T = AF(-n, -z, r, 1)$ ;

and Binet proves the interesting remark that had Stirling added the solution of  $u' = \frac{r+z}{r+z+n} u$ , where  $u' = \frac{1}{T'}$ ,  $u = \frac{1}{T}$ , he would have obtained

$$A/T = F(n, -z, r+n, 1),$$

i. e. he would have established the Gaussian formula

$$F(a, b, c, 1) \times F(-a, b, c-a, 1) = 1.$$

## STIRLING'S SERIES

§ 15. On pages 135-8 are given the formulae which have rendered Stirling's name familiar whenever calculations involving large numbers are concerned.

## STIRLING'S THEOREM

When  $n$  is a large number the product

$$1 \cdot 2 \cdot 3 \dots n = n^n \sqrt{2n\pi} e^{-n + \frac{\theta}{12n}},$$

where  $0 < \theta < 1$ .

Stirling actually gives the formula

$$\begin{aligned} \log (1 \cdot 2 \cdot 3 \dots x) &= \frac{1}{2} \log (2\pi) + (x + \frac{1}{2}) \log (x + \frac{1}{2}) \\ &\quad - (x + \frac{1}{2}) - \frac{1}{2 \cdot 12 \cdot (x + \frac{1}{2})} + \frac{7}{8 \cdot 360 (x + \frac{1}{2})^3} - \dots, \end{aligned}$$

with the law for the continuation of the series.

De Moivre (*Supp. Misc. Anal.*) later expressed this result in the more convenient form

$$\begin{aligned} \log (1 \cdot 2 \dots x) &= \frac{1}{2} \log (2\pi) + (x + \frac{1}{2}) \log x \\ &\quad - x + \frac{B_1}{1 \cdot 2} \frac{1}{x} - \frac{B_2}{3 \cdot 4} \frac{1}{x^3} + \dots \\ &\quad + (-1)^{n+1} \frac{B_n}{(2n-1)2^n} \frac{1}{x^{2n-1}} \dots \end{aligned}$$

Cauchy gave the remainder after the last term quoted as

$$R_n = \frac{(-1)^n \theta B_{n+1}}{(2n+1)(2n+2)} \frac{1}{x^{2n+1}}.$$

( $B_1, B_2$ , &c., denote the Bernoullian numbers.)

More particularly the series<sup>1</sup>

$$\frac{B_1}{1 \cdot 2} \frac{1}{x} - \frac{B_2}{3 \cdot 4} \frac{1}{x^3} + \dots$$

has been called the *Series of Stirling*. It is one of the most remarkable in the whole range of analysis to which quite a library of mathematical literature has been devoted. The series is divergent, and yet, in spite of this fact, when  $n$  is very large and only a few of the initial terms are taken, the approximation to  $\log n!$  found by it is quite suitable for practical purposes.<sup>1</sup> Its relative accuracy is due to the fact

<sup>1</sup> See Godefroy, *Théorie des Séries*, or Bromwich's *Treatise on Series*.

that the error committed at any stage, by neglecting  $R_n$ , is always less in absolute value than the first of the terms neglected, which suggests that the series should be discontinued when the minimum term is reached. Legendre has shown that if we write the series as  $\sum (-1)^{n+1} u_n$ , then

$$u_{n+1}/u_n < (2n-1)2n/4\pi^2 x^2,$$

and

$$\therefore < (n/3x)^2.$$

The terms therefore decrease so long as  $n$  does not exceed  $3x$ . When  $n = 3x$  the error is less in absolute value than

$$\cdot 393409 \dots \times x^{-\frac{1}{2}} e^{-6x}.$$

To later mathematicians, such as Gauss, who admitted only the use of convergent series, Stirling's Series was an insoluble riddle, but it now finds its place among the series defined as *Asymptotic Series*.<sup>1</sup>

To meet the objection to its divergence Binet (l. c., p. 226) gave the *convergent* representation.

$$\log (x-1)! = \frac{1}{2} \log (2\pi) + (x-\frac{1}{2}) \log x - x \\ + \frac{1}{2} \left[ \frac{1}{2 \cdot 3} S_2 + \frac{2}{3 \cdot 4} S_3 + \frac{3}{4 \cdot 5} S_4 + \dots \right]$$

in which  $S_n$  denotes  $\frac{1}{(x+1)^n} + \frac{1}{(x+2)^n} + \dots$  ad  $\infty$ .

From this by the use of inverse factorials he deduces (p. 231)

$$\log (x-1)! = \frac{1}{2} \log (2\pi) + (x-\frac{1}{2}) \log x - x$$

$$+ \frac{1}{12(x+1)} + \frac{1}{12(x+1)(x+2)} \\ + \frac{59}{360} \frac{1}{(x+1)(x+2)(x+3)} \\ + \frac{227}{480(x+1)\dots(x+4)} + \&c.$$

§ 16. The conclusion of Stirling's book is taken up with various problems in interpolation, based partly on a paper by him in the *Philosophical Transactions* for 1719, and partly

<sup>1</sup> Vide Poincaré, *Acta Math.*, 1886.

on the researches of Newton and Cotes. It may be noted that in Prop. XXX he gives the expression of one of the roots of a system of  $n$  linear equations in  $n$  variables, found 'per Algebram vulgarem'.

A translation into English by Francis Holliday was published in 1749 'with the author's approbation'.

There was also a second edition of the original treatise in 1764.

(C)

## CONTRIBUTIONS TO THE PHILOSOPHICAL TRANSACTIONS

§ 17. Though Ramsay (loc. cit.) refers to writings by Stirling while in Italy, I am not acquainted with any such, save the first of his three papers printed in the *Philosophical Transactions*.

It is entitled *Methodus Differentialis Newtoniana Illustrata Authore Iacobo Stirling, e Coll. Balliol. Oxon.*, and furnishes a useful commentary on Newton's *Methodus Differentialis* published in 1711. Stirling restricts his attention entirely to the case of equal increments and proves the three Interpolation Formulae already referred to above (p. 40). He deduces a number of special formulae, several of which are reproduced in his book of 1730. One of these may be noted on account of the uncanny accuracy of its approximations in certain cases.

Let  $\alpha, \beta, \gamma, \delta, \dots$  be a series of quantities, and write down the equations found by equating the differences to zero.

$$\alpha - \beta = 0,$$

$$\alpha - 2\beta + \gamma = 0,$$

$$\alpha - 3\beta + 3\gamma - \delta = 0, \text{ \&c.}$$

The assumption of any one of these will furnish a linear equation in  $\alpha, \beta$ , &c., from which any one of these may be determined when all the others are known:

e. g. to determine  $\int dz / (1 + z^2),$

consider  $(1 + z^2)^{-1}, (1 + z^2)^0, (1 + z^2)^1, \text{ \&c.}$

The integrals of these omitting the first, are  $z$ ,  $z + z^3/3$ , &c. Take the latter as  $\beta$ ,  $\gamma$ , &c., so that

$$\alpha = \int dz / (1 + z^2).$$

The above equations give in succession

$$\tan^{-1} z = z; \quad z - z^3/3; \quad z - z^3/3 + z^5/5, \text{ \&c.}$$

Other examples are easily constructed.

Towards the end of his paper, while discussing a method of approximating to a slowly converging series, he furnishes what seems to be one of the earliest general tests for the convergence of a series.

Consider the series of positive terms

$$u_1 + u_2 + \dots$$

If, in the long run,

$$\frac{1}{u_n} - \frac{1}{u_{n+1}} > \frac{1}{u_{n+1}} - \frac{1}{u_{n+2}},$$

the series is convergent; otherwise it is divergent.

There are also the two papers on the *Figure of the Earth*, and on *Stirling's Engine*, to which reference has already been made.

LETTER FROM STIRLING TO DE MOIVRE PRINTED IN THE  
*Miscellanea Analytica*.<sup>1</sup>

(De Moivre was naturally much surprised by the introduction of  $\pi$  into the calculation of the ratio of the coefficient of the middle term in  $(1+x)^n$  to the sum of all the coefficients. Cf. p. 172.)

Quadrinium circiter abhinc, vir Cl. cum significarem D. Alex. Cuming Problemata de Interpolatione & Summatione Serierum aliaque eius generis quae sub Analysisi vulgo recepta non cadunt, solvi posse per Methodum Differentialem Newtoni; respondit Illustrissimus vir se dubitare an Problema a te aliquot ante annos solutum de invenienda Uncia media in quavis dignitate Binomii solvi posset per Differentias. Ego dein curiositate inductus, & confidens me viro de Mathesi bene merito gratum facturum, idem libenter aggressus sum:

<sup>1</sup> *Miscellanea Analytica de Seriebus*, pp. 170-2.

& fateor ortas esse difficultates quae impediere quominus ad optatam conclusionem confestim pervenire potuerim, sed laboris haud piget, siquidem tandem assecutus sum solutionem adeo tibi probatam ut digneris eam propriis tuis scriptis inserere. Ea vero sic se habet.

Si Index Dignitatis sit numerus par, appelletur  $n$ ; vel si sit impar, vocetur  $n-1$ ; eritque ut Uncia media ad summam omnium eiusdem Dignitatis, ita unitas ad medium proportionale inter semi-circumferentiam Circuli & Seriem sequentem

$$x + \frac{A}{2 \times n + 2} + \frac{9B}{4 \times n + 4} + \frac{25C}{6 \times n + 6} + \frac{49D}{8 \times n + 8} + \frac{81E}{10 \times n + 10} \&c.$$

Exempli gratia, si quaeratur ratio Unciae mediae, ad summam omnium in Dignitate centesima vel nonagesima nona, erit  $n = 100$  qui ductus in semiperipheriam Circuli 1.5707963279 producit  $A$  primum terminum Seriei; dein erit

$$B = \frac{1}{204} A, \quad C = \frac{9}{416} B, \quad D = \frac{25}{636} C \&c,$$

atque perficiendo computum ut in margine, invenietur summa Terminorum 157.866984459, cuius Radix quadrata 12.5645129018 est ad unitatem ut summa omnium Unciarum ad mediam in Dignitate centesima, vel ut summa omnium ad alteram e mediis in Dignitate nonagesima nona.

Problema etiam solvitur per reciprocam illius Seriei, etenim summa omnium Unciarum est ad Unciam mediam in subduplicata ratione semiperipheriae Circuli ad Seriem

$$\frac{1}{n+1} + \frac{A}{2 \times n + 3} + \frac{9B}{4 \times n + 5} + \frac{25C}{6 \times n + 7} + \frac{49D}{8 \times n + 9} + \frac{81E}{10 \times n + 11} \&c.$$

vel quod eodem redit, ponatur  $a = .6366197723576$ , quoto scilicet qui prodit dividendo unitatem per semiperipheriam Circuli; & media proportionalis inter numerum  $a$ , & hanc Seriem, erit ad unitatem, ut Uncia media ad summam omnium.



Et hæc summa verum superat binario in ultima figura; estque logarithmus numeri 37·6098698 qui est ad unitatem ut Summa Unciarum ad mediam in dignitate 900 vel 899.

Et si vis illius numeri reciprocum, sume complementum logarithmi, scilicet—2·4246981692, & numerus eidem correspondens inveniatur .0265887652.

Et hæc sunt Solutiones quæ prodierunt per Methodum Differentialem Newtoni; quarum demonstrationes jam non attingo, cum in animo sit brevi publico impertire Tractatum quem de Interpolatione & Summatione serierum conscripsi.

*Tui Studiosissimi*

*19 Jun. 1729*

*Jac. Stirling*

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„ *Miscellanea Analytica de Seriebus*. 1730.
- (22) R. Reiff: *Geschichte der Unendlichen Reihen*. 1889.
- (23) I. Todhunter: *History of Probability and History of Attraction and the Theory of the Figure of the Earth*.
- (24) Any student wishing to study Stirling's methods cannot do better than read in the following order:
  - (i) J. Binet: *Mémoire sur les Intégrales Eulériennes*; Jour. École Poly. 1839.
  - (ii) N. Nielsen: *Theorie der Gammafunktion*. Teubner, 1906.  
 Also: *Les Polynomes de Stirling*. Copenhagen, 1820.
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STIRLING'S  
SCIENTIFIC CORRESPONDENCE



## INTRODUCTION

MUCH of the correspondence of James Stirling has been preserved at the family seat of Garden. In the collection are several letters from him to his friends in Scotland, and numerous extracts from them are to be found in the Family History:—*The Stirlings of Keir and their Private Papers*, by W. Fraser (Edinburgh, privately printed, 1858). In addition to these are letters of a scientific character which were with great courtesy placed at my disposal by Mrs. Stirling in 1917. Of the latter group of letters the earliest is one from Nicholas Bernoulli in 1719, and the last is one from M. Folkes, P.R.S. in 1747. Stirling enjoyed the acquaintance of most of the British mathematicians of his day, while his reputation and continental experience brought him into correspondence with continental scholars like Clairaut, Cramer, and Euler.

It is interesting to note that all of his correspondents save Campailla were, or became Fellows of the Royal Society of London. (It is clear from letter XI<sub>1</sub> that Stirling suggested to Euler that he should become a Fellow.) The dates when they joined are indicated in the notes added to the letters.

One learns from the letters how much depended on correspondence for the discussion of problems and the diffusion of new ideas, just as one would turn nowadays to the weekly and monthly journals of science. Several of the letters in the collection shed a good deal of light upon obscure points in the history of Mathematics, as indicated in the notes. Maclaurin appears to have been Stirling's chief correspondent and the letters between the two men are of particular interest to students of Scottish Mathematics. They were warm friends, though probably in opposite political camps, and Maclaurin had the benefit of Stirling's judgment when engaged upon his *Treatise of Fluxions*.

There are not many letters of Stirling, and those are chiefly copies made by Stirling himself.

I had the good fortune to find four original letters from Stirling to Maclaurin in the Maclaurin MSS. preserved in Aberdeen, and they fit in admirably with the letters of the Garden collection. But I am convinced that other letters by Stirling are still to be found. Stirling is known to have had frequent correspondence with R. Simson, G. Cramer, and De Moivre, not to mention others, and the discovery of fresh letters might be the reward of careful search. Among letters of Stirling already published may be mentioned his letter to Newton in 1719 (Brewster's *Newton*), a letter to J. Bradley reproduced in the *Works and Correspondence of Bradley*, a letter to De Moivre in the *Miscellanea Analytica de Seriebus*, and reference to a second letter in the Supplement to the same work.

Cramer's Letter III<sub>3</sub> and the letter from Stirling to Castel V<sub>2</sub> are reproduced in the Stirling Family History.

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which has been of great use to me in demonstrating easily many rules in algebra which I am afraid may be made use of in the paper you have printed because my dictates go through every body's hands here. The Observation is transform any Equation  $x^3 - px^2 + qx - r = 0$  to another that shall have its roots less than the values of  $x$  by any differences  $e$ ; let  $y = x - e$  and

$$y^3 + 3ey^2 + 3e^2y + e^3 = 0$$

where any coefficient considered as an Equation gives for its roots the limits of the following Coefficients

$$\begin{array}{r} -p \\ +q \\ -r \end{array}$$

considered as an Equation. This holds in Equations of all sorts and from this I demonstrate many rules in a very easy manner. By it too I demonstrate a Theorem in a book where a Quantity is expressed by a series whose coefficients are first, second, third fluxions &c. I shall be vexed a little if he has taken this from me. Pray let me know if there is any thing of this in the paper you have printed.

I intended to have sent you one of my Theorems about the collision of many Bodies striking one another in different directions in return for your admirable series. But I must leave that to another occasion. I expect to dispose of the six subscriptions I took for Mr De Moivre's Book. Please to give my humble service to Mr Machin & communicate what is above. I long for his new Theory. I am with great Respect

Doubling Dec: 7  
1728

Your most Obedient & Humble servant  
Colin Mac Laurin

I

COLIN MACLAURIN AND STIRLING

(1)

*Maclaurin to Stirling, 1728*

Mr James Stirling  
at the Academy in  
little Touer Street  
London

Sir

Your last letter was very acceptable to me on several Accounts. I intend to set about publishing the piece on the Collision of Bodys very soon. I was obliged to delay it till now having been very busy taking up my Classes in the College. Your remarks on their experiments are certainly just. I intend if I can get a good opportunity by any of our members of parlia<sup>t</sup> to send you a copy of my remarks before I publish them. I have seen Roberts's paper since I came from Perthshire in August where I writ my remarks and find he has made some of the same observations as I had made; nor could it well happen otherwise. I wish I had Mr Graham's Experiment at full length with Liberty to insert it. I design to write to him about this. I am much obliged to you for your kind offer and would accept of it if I was to publish this piece at London.

I spoke to Col. Middleton and some others of influence here and find they have better hopes of success to . . . Mr Campbell in that Business than you have

I think some of his performances deserved to be taken notice of. But as there is an imperfect piece of mine in the transactions for 1726 on the same subject I wish you had rather chose to publish some other of his pieces. I have been at pains to soften some prejudices and Jealousies that may possibly revive by it. It is true I have too long delayed

publishing the remainder of my piece for which I have only the excuse of much teaching and my design of giving a Treatise of Algebra where I was to treat that subject at large.

I told you in my last I had the method of demonstrating that rule by the Limits. In one of my Manuscripts is ye following Article.

$$\text{Let} \quad x^n - px^{n-1} + qx^{n-2} - rx^{n-3} \&c. = 0$$

be any equation proposed; deduce from it an Equation for its Limits

$$nx^{n-1} - \overline{n-1} \times px^{n-2} + \overline{n-2} \times qx^{n-3} \&c. = 0$$

and from this last deduce an equation for its limits; and by proceeding in this manner you will arrive at the quadratick

$$n \times \overline{n-1} \times x^2 - 2(n-1)px + 2q = 0$$

whose roots will be impossible if  $\frac{n-1}{2n} p^2$  be less than  $q$  and therefor in that case at least two roots of ye proposed Equation will be impossible. Afterwards I shew that if  $\frac{2}{3} \frac{n-2}{n-1} \times q^2$  be less than  $pr$  two roots must be impossible by a quadratick equation deduced a little differently, and so of the other terms. But this matter is so easy I do not think it worth while to contend about it. I have some more concern about a remark I make in my Algebra on the transformation of Equations which has been of great use to me in demonstrating easily many rules in Algebra which I am afraid may be made use of in the paper you have printed because my dietates go through everybody's hands here.

The Observation is transform any Equation

$$x^3 - px^2 + qx - r = 0$$

to another that shall have its roots less than the values of  $x$  by any difference  $e$ :

Let  $y = x - e$  and

$$y^3 + 3ey^2 + 3e^2y + e^3 = 0$$

$$-py^2 - 2pey - pe^2$$

$$+ qy + qe$$

$$- r$$

where any Coefficient considered as an Equation gives for its roots the limits of the following Coefficient considered as an Equa-

tion. This holds in Equations of all sorts and from this I demonstrate many rules in a very easy manner.

By it too I demonstrate a Theorem in y[our] (?) book where a Quantity is expressed by a series whose coefficients are first, second, third fluxions, &c. I shall be vexed a little if he has taken this from me. Pray let me know if there is any thing of this in the paper you have printed.

I intended to have sent you one of my Theorems about the Collision of many Bodys striking one another in different directions in return for your admirable series. But I must leave that to another occasion.

I expect to dispose of the six subscriptions I took for Mr De Moivre's Book. Please to give my humble service to Mr Machin and communicate what is above. I long for his new Theory. I am with great Respect

Sir

Your most Obedient and Humble Servant

COLIN MACLAURIN

Edinburgh Dec<sup>r</sup> 7

1728.

(2)

*Stirling to Maclaurin, 1728*

Sir

A few days ago I received your letter of the 7<sup>th</sup> of this Moneth and am very glad that your Book is in so great a forwardness, but you have never yet told me in what language it is, altho at the same time I question not but it is in Latine. I should be very glad to see what you have done, and since you mention sending a Copy, you may send it under Cover to Mr Cuninghame of Balghane; if I can do you any service as to getting Mr Grahams Experiment I wish you would let me know, I question not but that you may have liberty to print it, because probably it will be in our Transactions very soon.

I am very glad that Coll. Middleton gives Mr Campbell encouragement to come to London, no doubt but bread might

be made by private teaching if a man had a right way of mak[ing himself] known, but indeed I [ques]tion if Mr Campbel will not want a prompter in that p[ar]t. I am apt to thi[n]k that I ha[ve] not given you a distinet account of his paper about in [ ]<sup>1</sup> because you se[em to thi]nk that I choose it out of a great many others to be printed [ ] which indeed would not have been so very candid before you had leasure to compleat your paper. But the Matter is quite otherways. For as soon as your paper was printed, Mr Campbel sent up his directly to Mr Machine, who at that time being very busy, delayed presenting it to the Society because the Correcting of Press would divert him from prosecuting his Theory of the Moon. Upon this delay Sir Alex. Cuming complained grievously to Mr Machine that Mr Campbel was ill used, this made Mr Machine present it to the Society, upon which it was ordered to be printed, Mr Machine came to me and desired I would take the trouble of correcting it in the Press, which was all the Concern I had in it. And now I hope you are convinced that I did no more than yourself would have done had you been asked. Mr Campbels Method is grounded on the following observation. Let there be two equations  $x^5 + Ax^4 + Bx^3 + Cx^2 + Dx + E = 0$  and  $Ez^5 + Dz^4 + Cz^3 + Bz^2 + Az + 1 = 0$ , where the reciprocals of the Roots of the one are the Roots of the other, then it is plain that the Roots in both are the same as to possibility and impossibility. He deduces from each of those a Quadratick Equation for the limits the common way, and on that founds his Demonstration. But he doth not use that property of equations which you have been pleased to communicate, indeed it is very simple and I can see at once what great use can be made of it, I had observed that the last Term but one gave the Fluxion of the equation, but never any further before you mentioned it. But Mr Campbell besides demonstrating Sir Isaac Rule [ ] one of his own more general, he exemplifies it by an equation of 7 dimen[ ]ich his Rule discovers to have 6 impossible Roots, whereas Sr Isaac's disco[ ]ly two of the Six.

[I] shal now make a remark on some of those Gentlemen who dispute for the new [n]otion of Force to shew how

<sup>1</sup> Impossible roots (?).

much they depend one anothers demonstrations which are to convince their Adversarys.

Herman in his book page 113, I mean his *Phoronomia*, says *In hac virium æstimatione, præeuntem habemus Illustrissimum Leibnitium, qui eundem non uno loco in Actis eruditorum Leipsiæ indicavit quidem non tamen demonstravit, etsi apodictice demonstrari potest, ut forte alia id occasione ostendemus*—He denys then that his friend Leibnitz ever did demonstrate it, but owns that it may be done and is in hope one time or other to do it himself.

Poleni in his *Book de Castellis* page 49 tells us that Leibnitz demonstration was published; and page 52 he mentions Bernoulli demonstration [ ] as published in Wolfius. And page 53 [ ] that perhaps some and those not the most scrupulous might doubt [ ] Leibnitz's and Bernoullis demonstrations, and then page 61 he tells—is meaning in plain words, *Demonstrationem inventam fuisse reor non tamen editam*. So that it is very remarkable that a certain number of men should run into an opinion; and all of them deny one another's proofs. For Herman denys Leibnitz demonstration, and Poleni denys all that ever were given, and declares further that he knows not possibly on what principles one should proceed in such a Demonstration, but at the same time, he resolves to be of the opinion: whether it be proved or not. But no doubt you have observed many more of their Absurdities as well as this. I have not seen Mr Machin since I got your letter, but shal carry him your complements, I am afraid it will be long before wee see his Theory, for Mr Hadly and he do not agree about some part of it. We expect in the first Transaction Mr Bradley's account of the new motion observed in the fixt Stars. I wish you good success, and hope to see your book soon, I am with all respect Sir

London

Your most obedient

31 December

humble servant

1728

JAMES STIRLING

(3)

*Maclaurin to Stirling, 1729<sup>1</sup>*

Mr James Stirling  
 at the Academy in  
 little Tower Street  
 London.

Sir

Last tuesday night I saw the philosophical Transactions for the month of October for the first time. You may remember I wrote to you some time ago wishing some of Mr Campbell's papers might be taken notice of. I did not indeed then know that Mr Machin had any paper of his on the impossible roots. But even when I heard of it from you I was not much concerned because from a conversation with the Author on the street I concluded his method was from the equations for the Limits and never suspected that he had followed the very track which I had mark'd out in my paper in the transactions for May 1726 from the principle that the squares of the differences of Quantities are always positive as he has done in the latter part of this paper. As I never suspected that he had followed that Method I had no suspicion that he would prevent me in a Theorem that can be only obtained that way but cannot be overlooked in following that track. I cannot therefor but be a little concerned that after I had given the principles of my method and carried it some length and had it marked that my paper was to be continued another pursuing the very same thought should be published in the intervall; at least I might have been acquainted that I might have sent the continuation of mine before the other was published.

You would easily see that the latter part of Mr Campbell's paper after he has done with the limits is the very continuation of my theorems if you had the demonstrations.

Let there be any Equation

$$x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - Ex^{n-5} + Fx^{n-6} - Gx^{n-7} \\ + Hx^{n-8} - Ix^{n-9} + Kx^{n-10} - Lx^{n-11} + Mx^{n-12} \text{ \&c. } = 0$$

<sup>1</sup> 1728 O.S.; but 1729 N.S., cf. Letter I<sub>6</sub>.

and  $\frac{m-1}{2m} \times D^2$  will always exceed  $EC - FB + GA - H$

if  $m = n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \frac{n-3}{4}$  &c.

till you have as many factors as there are terms in the Equation preceeding  $D$ .

I have had this Theorem by me of a long time: and it easily arises from my Lemmata premised to my paper in the Transactions for May 1726. An abridgment of my demonstration as I have it in a book full of Calculs on these subjects is as follows. The square of the coefficient of  $D$  consists of the squares of its parts and of the double products of those parts multiplyed into each other. Call the sum of the first of these  $P$  the sum of the products  $Q$  and  $D^2 = P + 2Q$ . Now the number of those parts is  $m$  and therfor by the 4<sup>th</sup> Lemma of the paper in the transactions for May 1726  $(m-1)P$  must be greater than  $2Q$  and  $D^2 (= P + 2Q)$  must be greater than  $\frac{2m}{m-1} Q$  or  $\frac{m-1}{2m} D^2$  greater than  $Q$ . Then I shew that

$$Q = EC - FB + GA - H$$

and thence conclude that  $\frac{m-1}{2m} D^2$  always exceeds

$$EC - FB + GA - H$$

when the roots of the equation are all real.

I have a general Theoreme by which I am enabled to compare any products of coefficients with any other products of the same dimensions or with the Sums and Differences of any such products which to shew you how much I have considered this subject tho' I have been prevented when I thought myself very secure I now give you. Let  $E$  and  $H$  be any two coefficients and  $m$  the number of Terms from  $E$  to  $H$  including both then shall

$$EH = P + \overline{m+1} Q + \frac{m+2}{1} \cdot \frac{m+3}{2} R + \frac{m+4}{1} \cdot \frac{m+5}{2} \cdot \frac{m+6}{3} S \\ + \frac{m+7}{1} \frac{m+8}{2} \frac{m+9}{3} \frac{m+10}{4} T \text{ \&c.}$$

where  $P$  expresses the squares of the parts of  $E$  multiplyed

by the dissimilar parts of  $C$ . (a term as far distant from the beginning of the Equation as  $H$  is from  $E$ )  $Q$  expresses the squares of the parts of the coefficient immediately preceding  $E$  viz.  $D$  multiplied by the dissimilar parts of the term next following  $C$  but one viz. in this case  $E$  itself.  $R$  expresses the squares of the parts of the coefficient next preceding  $E$  but one that is  $C$  multiplied by the dissimilar parts of the Term next following  $C$  but three viz.  $G$ ; and so on. Where I mean by the parts of a coefficient the terms that according to the common Genesis of Equations produce it; and by dissimilar parts those that involve not the same Quantities.

This general Theorem opens to me a vast variety of Theorems for comparing the products or squares of coefficients with one another of which those hitherto published are only particular Examples. Here I give you the theorem for comparing any two products of the same dimensions as  $EI$  and  $CL$ . Let  $s$  and  $m$  express the number of terms that precede  $C$  and  $I$  in the Equation then let

$$p = \frac{n-1}{s+1} \times \frac{n-s-1}{s+2} \times \frac{n-s-2}{s+3} \text{ \&c.}$$

and 
$$q = \frac{n-m}{m+1} \times \frac{n-m-1}{m+2} \times \frac{n-m-2}{m+3} \text{ \&c.}$$

continued in each till you have as many factors as there are terms from  $C$  to  $E$  including one of them only; then shall  $\frac{q}{p} \times EI$  always exceed  $CL$  when the roots are all real.

Then I proceed to compare the products of the Coefficients with the sums or differences of other products & one of the chief Theorems in that part is that mentioned above which Mr Campbell also found by the same method as is very apparent and could not miss in following the track I mark'd out in the transactions.

I had observed that my rules gave often impossible roots in the Equations when Sir Isaac's did not in proof of which I faithfully transcribe from my Manuscript the following Article.

'In the Equation

$$x^5 - Ax^4 + Bx^3 - Cx^2 + Dx - E = 0$$

$$x^5 - 10x^4 + 30x^3 - 44x^2 + 32x - 9 = 0$$

no impossible roots appear by Sir Isaac's rule. But  $B^2 \times \frac{m-1}{2m}$  here is less than  $AC-D$  for

$$m = n \times \frac{n-1}{2} = 5 \times \frac{4}{2} = 10 \quad \text{and} \quad \frac{m-1}{2m} = \frac{9}{20}$$

now  $\frac{9}{20} \times 30 \times 30$  is less than  $44 \times 10 - 32$  the first being 405 the latter 408 so that there must be impossible roots by our rule.'

After that I give other Examples

I believe you will easily allow I could not have invented these Theorems since tuesday last especially when at present by teaching six hours daily I have little relish left for such investigations. I showed too my theorems to some persons, who can witness for me. But I am afraid these things are not worthy your attention. Only as these things once cost me some pains I cannot but with some regret see myself prevented. However I think I can do myself sufficient justice by the length I have carried the subject beyond what it is in the transactions.

I believe you will not find that Mr Campbell sent up his paper or at least the latter part of it so soon after I sent up mine which was in the beginning of 1726. One reason I have is that Mr Machin never mentioned it to me tho' I spent a whole day with him in September 1727 and talked to him on this subject and saw some other papers of Mr Campbell's in his hand at that time. So that I have ground to think that the paper of May 1726 led the Author into the latter part of his for October 1728.

When I was with Mr Machin in September 1727 I then had not found a sufficient demonstration for the cases of Sir Isaac's rule when there may be six or seven impossible roots arising by it. This part is entirely overlooked by this Author: for all he demonstrates amounts only to some properties of Equations that have all their roots real; from which he says indeed all Sir Isaac's rule immediately follows. But I conclude from thence that he did not try to demonstrate compleatly Sir Isaac's rule. If he had tryed it new difficultys would have arisen which he has not thought of.

The way he has taken to demonstrate Sir Isaac's numbers

from the Limits is not so simple as that I have which I may send you again.

I now beg pardon for this long letter which I beg you would communicate to Mr Machin not by way of complaint against him for whom I have more respect than for any Mathematician whatsoever; but to do me justice in the matter of these impossible roots which I had thrown aside for some time and have now taken up with regret. I would have justice done me without disputing or displeasing anybody. At any [rate] in a few days I shall be very easy about the whole Matter. I am with the greatest Respect

Sir

Your Most Obedient

Affectionat Humble Servant

Edinburgh

COLIN MACLAURIN

febr. 11. 1728

Having room I send you here one of my Theorems about the Collision of Bodys.

Let the Body  $C$  moving in the direction  $CD$  strike any number of Bodys of any magnitude  $A, B, E, F$ , &c. and make

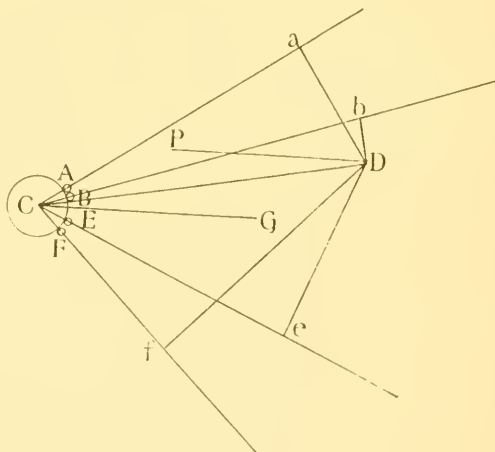


FIG. 4.

them move in the lines  $Ca, Cb, Ce, Cf$  &c. to determine ye direction of  $C$  itself after the stroke.

Suppose  $Da$ ,  $Db$ ,  $De$ ,  $Df$  &c. perpendicular to the directions  $CA$ ,  $CB$ ,  $CE$ ,  $CF$ , &c. Imagine the Bodys  $C$ ,  $A$ ,  $B$ ,  $E$ ,  $F$  &c. to be placed in  $C$ ,  $a$ ,  $b$ ,  $e$ ,  $f$  &c. respectively; find the centre of Gravity of all those Bodys so placed and let it be  $P$ . Draw  $DP$  and  $CG$  parallel to  $DP$  shall be ye direction of  $C$  after the stroke if the Bodys are perfectly hard.

Adieu

(4)

*Maclaurin to Stirling, 1729*

Mr James Stirling  
at the Academy in  
little Tower Street  
London

Sir

I delayed answering your last letter till I could tell you that now I have sent Mr Folkes the remainder of my paper concerning the impossible Roots of Equations. I sent him a part April 19 and the remainder last post. I thought to have finished it in our Vacation in March but a Gentleman compelled me to go to the Country with him all that time where we had nothing but diversions of one sort or other, so that I did not get looking into it once. However I am satisfied that any person who will read this paper and compare it with Mr Campbell's will do me Justice. On comparing them further myself I (find) he has prevented me in one proposition only; which I have stated without naming or citing him or his paper to be the least valuable. For I shew that some other rules I have deduced from my Theorems always discover impossible roots in an Equation when his rule discovers any, and often when his discovers none. I wish you could find time to read both the papers.

I am sorry to find you so uneasy about what has happened in your last letter. It is over with me. When I found one of my Propositions in his paper I was at first a little in pain; but when I found it was only one of a great many of mine

that he had hit upon; and reflected that the generality of my Theorems would satisfy any judicious reader; I became less concerned. All I now desire is to have my paper or at least the first part of it published as soon as possible. I beg you may put Mr Machin in mind of this. I doubt not but you and he will do what you can to have this Justice done me. I could not but send the second part to Mr Folkes having sent him the first.

I have at the end of my paper given some observations on Equations for the sake of those who may think the impossible roots may not deserve all this trouble. Mr Folkes will shew you the paper. I intend now to set about the Collisions of Bodys.

The Proposition I sent you in my last letter is the foundation of all my Theorems about the impossible Roots. I have a little altered the form of it. It is the VI Proposition as I have sent them to Mr Folkes the first five having been given in 1726. I have made all my Theorems as I went over them last and transcribed them more simple than they were in my manuscripts; and that occasioned this little delay: for your advice about sending up my paper soon perfectly pleased me. Abridgments and Additions that occurred as I transcribed it took up my time but it was about the third or fourth of April before I got beginning to it in earnest, and my teaching in the Colledge continuing still as before with other avocations; you will allow I have not lost time.

I have a particular sense of the Justice and kindness you have showed me in your last letter & will not forget it if I ever have any opportunity of showing with how much Esteem & affection

I am Sir

Your Most Obedient

Humble Servant

COLIN MACLAURIN

Edinburgh May 1

1729

(5)

*Maclaurin to Stirling, 1729*

Mr James Stirling  
 at the Academy  
 in little Tower Street  
 London

Sir

Since I received your last I have been mostly in the country. On my return I was surprised with a printed piece from Mr Campbell against me which the gentleman who franked the letter told me he sent you a copy off. The Gentleman indeed added he had not frank'd it if he had known the nature of the paper; and was ashamed of it.

I wonder I had no message by a good hand from Mr Campbell before he printed these silly reports he diverts himself with. Good manners and prudence one would think ought to have led to another sort of conduct.

He has misrepresented my paper much and found things in it I never asserted. I shall send you next post a fuller answer to it. His friends here give out that you desyred him to write against me. I am convinced this is false.

Please to send me the letter I wrote to you in february if you have preserved it or a copy of it. I wish if it is not too much trouble you would send me a copy of all I said relating to Mr Campbell's taking the hint from my first paper in my letters to you.

I wish you would allow me (if I print any defence) to publish your letter to me of the date of febr. 27 where you have expressed yourself very cautiously. But I will not do it without your permission.

I hope the paper Mr Campbell has sent you will have little influence on you till you hear my reply. I have writ at large to Mr Folkes by this post who will show you my letter if you please. I assure you I am with great Esteem

Sir

Your Most Obedient

Most Humble Servant

COLIN MACLAURIN

Edinburgh  
 nov<sup>r</sup> 6. 1729

(6)

*Stirling to Maclaurin, 1729*

To Mr Maclaurin Professor of Mathematicks  
in the University of  
Edinburgh

Out of your Letter of October 22, 1728

I have other ways of demonstrating the Rule about impossible roots & particularly one that was suggested to me from reading your book in 1718 drawn from the limits of Equations shorter than the one I have published, but according to my taste not so elegant.

Out of Letter of December 7, 1728

Let  $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} \&c. = 0$ , be any Equation proposed, deduce from it an Equation for its Limits

$$nx^{n-1} - \overline{n-1} \times px^{n-2} + \overline{n-2} \times qx^{n-3} \&c. = 0;$$

. . . . .

By it too I demonstrate a Theoreme in your book where a quantity is expresst by a Series whose coefficients are first, second, third fluxions &c.

A Copy of your Letter Feb 11, 1728.

Sr

Last Tuesday night I saw the philosophical Transactions for the month of October for the first time.

. . . . .

At any rate in a few days I shall be very easy about the whole matter. I am &c.

Sr This is an exact copy except the postscript which containing a Theoreme about the collision of Bodys I presume is nothing to the present purpose. I am with all respect

Sr

Your most humble servant

JA: STIRLING

London 29 November 1729

(7)

*Maclaurin to Stirling*

Dear Sir

I send you with this letter my answer to Mr George Campbell which I publish with regret being so far from delighting in such a difference that I have the greatest dislike at a publick dispute of this Nature. At the same time that I own this Aversion I can assure you it flows not from any Consciousness of any other wrong I have done this Author than that I accepted of a settlement here that was proposed to me when some persons at Aberdeen were persecuting me and when a settlement here every way made me easy; at the same time that he had some hopes tho' uncertain in a course of years of getting the same place.

I was sensible however of this and therefor made it my great Concern to see him settled ever since I have been in this place, nay after my business had proceeded so well that it was indifferent to me whether he continued here or not in respect of Interest.

However I have avoided everything that might seem writ in his strain and have left out many things lest they might look too strong. particularly in citing Mr Folkes's letter I left out his words that Mr Campbell's paper was writ with the greatest passion and partiality to himself, as you will see. I sent the first sheet in Manuscript to have been communicated to you above a fortnight ago by Mr Folkes that you might let me know if you desyred to have anything changed and have delayed the publication till I thought there was time for an Answer to come to me. I have printed but a few Copys intending only to take of as much (without hurting him)<sup>1</sup> the Impression he endeavours to make as possible.

It was to avoid little skirmishing that I have not followed him from page to page—but refuted the essentials of his piece, overlooking his Imaginations and Strictures upon them. I am at present in haste having several other letters to write on this subject. I avoid things together towards the

<sup>1</sup> Written above the line.

end because it was like to have required another half-sheet. I am sure I have given more than the subject deserves. I have left out two or three paragraphs about his inconsistencies his story of some that visited me and found me so and so engaged &c. This I answer in my manuscript letter sent to you, Nov. 5. I am indeed tyred with this affair.

I wished to have heard from you what he objected to the letter I wrote to you in the begining of winter. I am truly sorry Mr Campbell has acted the part he has pleased to act. But my defence is in such terms after all his bad usage of me as I believe to his own friends will shew I have no design to do him wrong and have been forced into this ungrateful part. It is true he speaks the same language; with what ground let the most partial of his friends judge from what I have said in my defence.

You may remember that my desyre of doing him service was what began our correspondence. I then could not have imagined what has happened. Please to forgive all the trouble I have given you on this Occasion and believe me to be Sir

Your Most Obedient

Humble Servant

COLIN MAC LAURIN

If you see Mr de Moivre soon, please to tell him I send him by this post a bill for six guineas and a letter directed to Slaughter's Coffee House. I did not know where else to direct for him.

(8)

*Gray to Maclaurin, 1732*

London 25 Novem<sup>r</sup> 1732

Dear Sir

I had the favour of yours yesterday & inclosed a part of the abstract of your Supplement with a Letter to Mr Machin, which, as you desired, I copyed & gave to him. He is of opinion that it will be improper to put any part of your Abstract into our Abridgment, especially as matters stand. He will take care to do you all the justice he can and desires

his kind services to you. I am thinking that it will not be improper to move the Society at their first meeting that Stirling be in Hodgson's room; because he is much more capable of judging than him; but in this I will follow Mr Machin's advice. I hope you had my last, and am persuaded you will do in that affair what is fit.

I have a great deal of business to do this evening. I will therefore only assure you that I am most faithfully

Dear Sir

Your most obedient  
& most humble Servant  
JNO GRAY

(9)

*Maclaurin to Stirling, 1734*

To

Mr James Stirling  
at Mr Watt Academy  
in little Tower Street  
London

Sir

I was sorry on several accounts that I did not see you again before you left this Country. Being in the Country your letter about the Variation did not come to my hand till the time you said you had fix'd for your journey was so near that I thought a letter could not find you at Calder.

I have observed it since I came to Town & found it betwixt 12 & 13 degrees westerly; the same had appeared in April last. But I am to take some more pains upon it which if necessary I shall communicate.

Upon more consideration I did not think it best to write an answer to Dean Berkeley but to write a treatise of fluxions which might answer the purpose and be useful to my scholars. I intend that it shall be laid before you as soon as I shall send two or three sheets more of it to Mr Warrender that I may have your judgment of it with all openness & liberty. This

favour I am the rather obliged to ask of you that I had no body to examine it here before I sent it up on whose judgment I could perfectly depend. Robt. Simpson is lazy you know and perhaps has not considered that subject so much as some others. But I can entirely depend on your judgment. I am not at present inclined to put my name to it. Amongst other reasons there is one that in my writings in my younger years I have not perhaps come up to that accuracy which I may seem to require here. When I was very young I was an admirer too of infinites; and it was Fontenelle's piece that gave me a disgust of them or at least confirmed it together with reading some of the Antients more carefully than I had done in my younger years. I have some thoughts in order to make this little treatise more compleat to endeavour to make some of Mr De Moivre's theorems more easy which I hope he will not take amiss as I intend to name everybody without naming myself.

I have got some few promises as to Mr Machin's book and one of my correspondents writes me that he has got two subscriptions. I wonder at Dr Smith's obstinate delay which deprives me of the power of serving Mr Machin as yet so much as I desyre to do. It is from a certain number of hands that I get subscriptions of this kind. Pemberton's book and the Doctor's delay diminish my influence in that very much.

Looking over some letters I observed the other day that you had once wrote to me you had got a copy from Mr Machin of the little piece he had printed on the Moon for me. If you can recollect to whom you sent it let me know; for it never came to my hand; and I know not how to get it here. Nor did the Copy of your treatise of Series come to my hand. You need not be uneasy at this: Only let me know what you can recollect about them. If Mr Machin's book happens to be published soon you may always venture to sett me down for seven Copys. But I hope to gett more if I had once fairly delivered Dr Smith's book to the subscribers. As to your Treatise of Series I got a copy sent me from one Stewart a Bookseller as a new book but about half a year after his son sent me a note of my being due half a guinea for it which I payed. But as I said I only mention these things in case you can recollect any thing further about them.

I observe in our newspapers that Dr Halley has found the longitude. I shall be glad to know if there is any more in this than what was commonly talk'd when I was in London in 1732. Please to give my humble service to Mr Machin and believe me to be very affectionatly

Sir

Your Most Obedient

Most Humble Servant

Edinburgh  
Nov<sup>r</sup>. 16. 1734.

COLIN MACLAURIN

I have taken the liberty to desyre Mr Warrender to take advice with you if any difficultys arise about the publishing the fluxions or the terms with a Bookseller. I would have given you more trouble perhaps but he was on some terms with me before you got to London.

(10)

*Maclaurin to Stirling, 1738*<sup>1</sup>

To

Mr James Stirling  
at Leadhills

Dear Sir

This is a copy of Maupertuis's letter which I thought it would be acceptable to you to receive. I am told Mr Cassini would willingly find some fault with the Observation to save his father's doctrine, but is so much at a loss that he is obliged to suppose the instrument was twice disordered. If I can be of any service to you here in anything you may always command

Dear Sir

Your Most Obedient

Humble Servant

Ed<sup>r</sup>. feb.<sup>r</sup> 4. 1737.

COLIN MAC LAURIN

I forgot when you was here to tell you that last spring

<sup>1</sup> 1737 O.S. or 1738 N.S.

some Gentlemen had formed a design of a philosophical society here which they imagined might promote a spirit for natural knowledge in this country, that you was one of the members first thought of, and that Ld Hope & I were desyred to speak to you of it. I hope and intreat you will accept. The number is limited to 45, of which are L<sup>ds</sup> Morton, Hope, Elphinston, St Clair, Lauderdale, Stormont, L<sup>d</sup> president & Minto, S<sup>r</sup> John Clark, D<sup>rs</sup> Clark, Stevenson, St Clair, Pringle, Johnston, Simpson, Martin, Mess. Munroe, Craw, Short, Mr Will<sup>m</sup> Carmichael &c. I shall write you a fuller account afterwards if you will allow me to tell them that you are willing to be of the number. If you would send us anything it would be most acceptable to them all & particularly to yours &c

I had a letter from Mr De Moivre where he desyres to give his humble service to you. His book was to be out last week.

*Maupertuis to Bradley*

A letter from Mons<sup>r</sup> Maupertuis

To Professor Bradley

Dated at Paris Sept<sup>r</sup> 27<sup>th</sup> 1737 N.S.

[Translated from the French]

Sir

The Rank You hold among the Learned & the great Discoveries with which you have enriched Astronomy, would obligé me to give you an Account of the Success of an Undertaking, which is of considerable consequence to Sciences (even tho' I were not moved to do it by my desire of having the honour to be known to you) by reason of the Share you have in the Work itself. Whereof a great part of the Exactitude is owing to an Instrument made on the Modell of yours, and towards the Construction of which I know you were pleased to lend your Assistance.

Wherefore I have the honour to Acquaint You Sir, That we are now returned from the Voyage we have made by Order of His Majesty to the Poler Circle. We have been so happy as

to be able, notwithstanding the Severity of that Climate, to measure from Tornea northward a Distance of 55023.47 Toises on the Meridian. We had this distance by a Basis the longest that ever has been made use of in this Sort of Work, & measured on the most level surface, that is, on the Ice, taken in the middle of eight Triangles. And the small number of these Triangles, together with the Situation of this great Basis in the Midst of them, Seem to promise us a great Degree of Exactness; And leave us no room to apprehend any considerable Accumulation of Mistakes; As it is to be feared in a Series of a greater Number of Triangles.

We afterwards determined the Amplitude of this Arch by the Starr  $\delta$  *Draconis*, Which we observed at each end with the Sector you are Acquainted with. This Starr was first observed over Kittis, one of the Ends, on the 1, 5, 6, 8, 10 of October 1736.

And then we immediately carried our Sector by Water to Tornea, with all the precaution requisite its being any way put out of Order, And we observed the same Starr at Tornea the 1, 2, 3, 4 & 5, of Nov<sup>r</sup> 1736. By comparing these two Setts of Observations we found, That the Amplitude of our Arch (without making any other Correction than that which The procession of the Equinox requires) would be  $57^{\circ}25'07''$ . But upon making the necessary Correction according to your fine Theory (Parallax of Light) of the Aberration caused by the Motion of Light, This Amplitude by reason of the interval of Time between the Mean of the Observations, was greater by  $1''83$ : & consequently our Amplitude was  $57^{\circ}27'09''$ .

We were immediately Sensible that a Degree on the Meridian under the Polar Circle was much greater than that which had been formerly measured near Paris.

In Spring of the ensuing Year we Recommenced this whole operation. At Tornea we observed Alpha *Draconis* on the 17, 18, & 19 of March 1737; and Afterwards set out for Kittis, Our Sector was this time drawn in a Sledge on the Snow, and went but a slow pace. We observed the Same Starr on the 4, 5 & 6 of Aprile 1737. By the Observations made at Tornea & Kittis we had  $57^{\circ}25'19''$ ; to Which Adding  $5''35$  for the Aberration of this Starr during the time elapsed between the Middle of the Observations, we found for the Amplitude

of our Arch  $57^{\circ}30''.51$  which differs  $3''\frac{1}{2}$  from the Amplitude determined by  $\delta$  (Delta).

Therefore taking a Mean between these two amplitudes, Our Arch will be  $57^{\circ}28''.72$  which being compared with the distance measured on the Earth, gives the Degree  $57437.1$  Toises; greater by  $377.1$  Toises than the Middle Degree of France.

We looked upon the Verification which results from the Agreement between our two Amplitudes deduced from two different (Setts of) Operations (Joined to the precautions we had taken in the Carriage of the Sector) We looked (I say) upon this Verification to be more certain than any other that could be made; and the more because our Instrument cannot from its Construction serve to be turned Contrary Ways. And that it was not requisite for our operation to know precisely the point of the Limb which answered to the Zenith.

We verified the Arch of our Instrument to be  $15^{\circ}\frac{1}{2}$  by a Radius of  $380$  Toises, and a Tangent both measured on the Ice: and notwithstanding the great Opinion we had of Mr Graham's Abilities we were astonished to see, that upon taking the Mean of the Observations made by  $5$  Observers which agreed very well together; The Arch of the Limb differed but  $1''$  from what it ought to be According to the Construction. In fine, we Compar'd the degrees of the Limb with one Another, and were surprized to find that between the two Degrees which we had made use of, there is a Small Inequality, Which does not amount to  $1''$ , & Which draws the two Amplitudes, we had found, Still nearer one Another.

Thus, Sir, You See the Earth is Oblate, according to the Actual Measurements, as it has been already [found] by the Laws of Staticks: and this flatness appears even more considerable than Sir Isaac Newton thought it. I'm likewise of Opinion, both from the experiments we Made in the frigid Zone, & by those Which our Academicians sent us from their Expedition to the Equator; that Gravity increaseth more towards the Pole, and diminishes more towards the Line, than Sir Isaac suppos'd it in his Table.

And this is all conformable to the Remarks you made on Mr Campbell's Experiments at Jamaica. But I have one

favour to beg of you, Sir, & hope you will not refuse it me; Which is, to let me know if you have any immediate Observations on the Aberration of our two Stars  $\delta$  &  $\alpha'$  *Draconis*; and if we have made proper Corrections for this Aberration.

I shall have the honour, at Some Other time to communicate to you our Experiments on Gravity, & the Whole detail of our Operations, as soon as published.

I have the honour to be with Sentiments of the highest Esteem

Sir

Your Most humble & most Obedient Servant

MAUPERTUIS

I shall be much obliged to you if you will be pleased to Communicate . . . the Royal Soc . . .

(11)

*Maclaurin to Stirling, 1738*

Mr. James Stirling  
at Lead Hills

Dear Sir

There is an ingenious young man here who I am very sure will please you for what you write about. I have promised him no more but that you will bear his charges in going & returning & give him some small thing besides perhaps. I have not omitted to acquaint him that he will have opportunity to improve himself with you. He is a quiet modest industrious & accurat young man. I think I have mentioned him to you as one who seems to have a natural turn for making mathematical instruments, & deserves encouragement. But his father is a poor minister who has ruined himself by lawsuits. If it will be time enough, it will be more convenient for him to go about the middle or end of May than just now.

I have a part of a letter I writ for you some weeks ago in town, but some incidents hindered me from finishing it.

## 8) STIRLING'S SCIENTIFIC CORRESPONDENCE

I shall write soon by the post. This goes by a student who is to leave it for you at the lead hills.

I am Dear Sir

Your Most Obedient

Dean near Edr

Humble Servant

April 1738.

COLIN MACLAURIN

Mr De Moivre's book is come but I have not had time to look much into it. I think you said you would send me Mr Machin's piece. I say a little of the centripetal forces but that part is now a printing off. Have you ever had occasion to enquire into the fluent of such a quantity as this

$$\frac{\dot{x}}{x \sqrt{a-x} \times \sqrt{b-x} \times \sqrt{c-x}}?$$

The common methods do not extend to it.

My family is now come to this place, but I go every day to town to the college. The removing & some incidents occasioned my delay in writing which I hope you will forgive.

(12)

*Maclaurin to Stirling, 1738*

To

Mr. James Stirling  
at Leadhills

Dear Sir

This is to introduce Mr Maitland whom I have dispatched sooner than I intended because of your urging it in a letter I received on Monday last. I heartily thank you for Mr Machin's piece, and that you may not be deprived of the book bound in with it I shall send you my copy of it.

I am persuaded many things are wanting in the inverse method of fluxions especially in what relates to fluents that are not reduced & perhaps are not reducible to the logarithms or circle. I give a chapter on these, distinguish them into various orders, and shew easy constructions of lines by whose

rectification they may be assigned, how to compare the more complex with the more simple & other things of this nature. But I suspect that some fluents (at least in some suppositions of the variable quantity) may be reduced to the circle or logarithms that are not comprehended in the cases that have been considered by Cotes & De Moivre.

I could not hit upon a letter I had writ a great part of to you in our vacation week when I sought for it today. I shall mention somethings of it as my memory serves.

I easily found as you observed that the right line  $AB$  attracts the particle  $P$  with the same force as the ark  $CED$  but I could make little use of this because when the figure revolves on the axis  $PE$ , the attractions of the circle generated by  $AE$  & of the spherical surface generated by  $CE$  are not equal.

I found that what I had observed long ago of the attraction of spherical surfaces holds likewise of what is included betwixt two similar concentric spheroidical surfaces infinitely near each other viz. That the attraction of the part convex towards the particle is equal to the attraction of the part concave towards it. This holds whether the particle be in the axis of the spheroid or not.

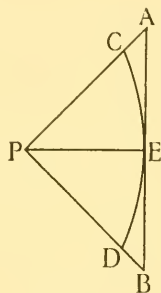


FIG. 5.

Let  $EGKL$  be any solid,  $P$  the particle attracted, let  $PEK$

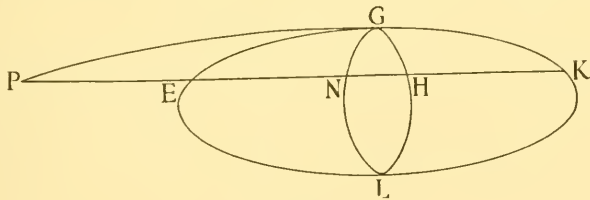


FIG. 6.

meet the solid in  $E$  &  $K$  and any surface  $GHL$  in  $H$ , let  $NH$  be to  $EK$  in any invariable ratio, and the point  $N$  form a surface  $GNL$ . Then the attraction towards the solid  $GNLH$  shall be to the attraction of the solid  $EGKL$  in the same given ratio of  $NH$  to  $EK$ .

Let  $ACE$  be a quadrant of a meridian,  $A$  the pole,  $E$  at the

equator, if  $LM$  be the direction of the gravity at  $L$  then  $CM$  shall be to the ordinate  $LP$  in an invariable ratio. This ratio

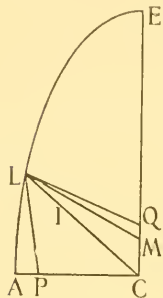


FIG. 7.

I cannot precisely recollect unless I had my papers which are at the Dean. I remember it is compounded of two ratios but how I cannot suddenly recollect. One of them I think is the ratio of the gravity at  $A$  to the force towards a sphere of the radius  $CA$ , the other is the ratio of the gravity at  $E$  to the force towards a sphere of the radius  $CE$ . I write this in a haste at the college because Mr Maitland waits for it and I do not incline to detain him.

On looking over the argument by which I had thought to have proved that the earth is a spheroid, I found that it supposed that in any right line  $CL$  from the center the gravity at  $L$  is to the centrifugal force as the gravity at  $l$  is to the centrifugal force. But this seems to need a proof. I have some more propositions, if they be worth your while I shall send them.

Having no time to go home for the book I was to send I delay it till some carrier call to whom I shall give it. If you will send me your receipt for De Moivre I shall cause one of the Booksellers get it down. In the mean time you may command my copy if you please. I am

Dear Sir

Your Most Obedient

Humble Servant

COLIN MACLAURIN.

Ed<sup>r</sup> May 12.

1738

(13)

*Stirling to Maclaurin, 1738*

Leadhills 13 May 1738

Dear Sir

I am obliged to you for dispatching Mr Maitland, for I am in a hast, & I hope he will do very well with smal assistance.

I shal be very glad to see what you have on fluents when your book comes out, particularly if you can reduce to the area of a Conick Section, figured not comprehended in the Theorems of S<sup>r</sup> Isaac, Cotes, or De Moivre, I readily agree with you that great improvements may be in that piece of knowledge; but the way to it is so rugged that I am afraid we are not in the right path.

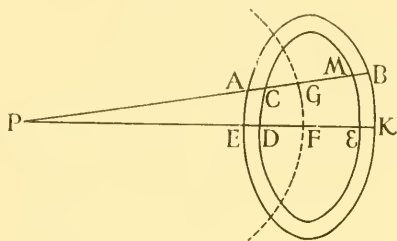


FIG. 8.

As to the attraction of an arch and its tangent being the same, on a particle placed in the center, it was of no use to me more than to you. What you say about the attraction of the concave and convex part of a spheroidical surface, being the same on a particle of matter, holds of any part of a spheroid comprehended betwixt two similar, concentric and similarly placed spheroidical surfaces, whether the distance betwixt them be infinitely smal or finite; Suppose two such surfaces to be  $AEKB$  and  $CDΞM$ , and a particle  $P$  placed any where; through  $P$  and  $F$  the center of the spheroid, imagine a spherical surface to be described similar and similarly placed with  $AEKB$ ; and that surface will cutt off the concave part from the convex part; and will divide the whole spheroid into two parts, whose attraction on  $P$  are equal; which is true whether the particle  $P$  be without or with the spheroid. The reason of it is because the ellipsis passing through  $P$  and  $F$ , cutts all the lines  $AB$  and  $EK$  into equal parts, if they converge to  $P$ . And from the same principle follows what you say next in your letter, about the attraction of solids being in a given proportion: because the solides may be divided into cones whose vertex is the particle attracted. And what you say about  $LP$  being in an invariable ratio to  $CM$  is true; but that ration cannot be assigned without

the quadrature of the circle. And the whole probleme about the variation of gravity on the Surface depends on it. When I first solved that problem, I supposed the attracted particle to be on the surface; but now I am upon solving it, when the particle is placed without the spheroid on any distance, which I have not had time yet to do, altho I know I am master of it; I have done it at the equator, I mean when the particle is in the plain of the equator produced; Newton did it when it was in the axis produced.

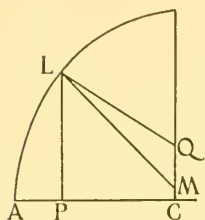


FIG. 9.

Suppose two ellipses similar described about the same center whose axes are  $EK$  and  $ek$ , and  $GL$  and  $gl$  the diameters of

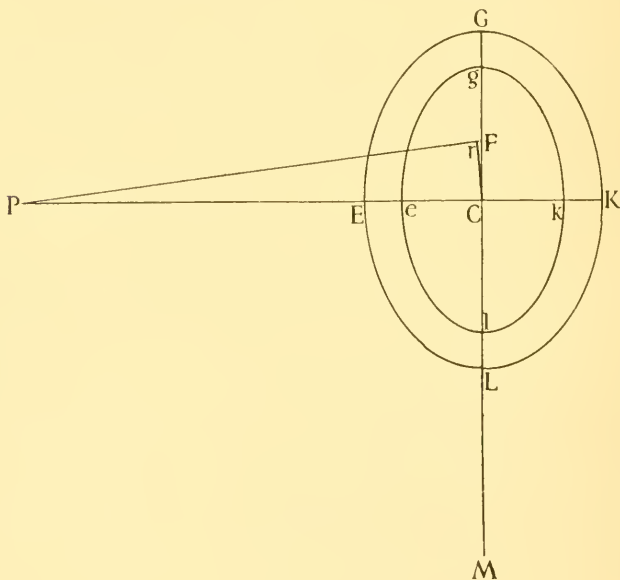


FIG. 10.

their equators whose difference I suppose infinitely little: Let  $F$  the focus and  $C$  the center; then if the elliptic ring revolve about the axis  $EK$  and generate a solid; and  $P$  be a particle in the axis produced, the gravitation of the particle  $P$  towards the solid comprehended betwixt the spheroidical surfaces will

be proportional to  $\frac{GL \times EK}{PF^2}$ : that is in a ratio compounded of the direct ratio of a rectangle under the axes, and in the duplicate inverse ratio of the distance of the particle from either of the foci: whence it follows that the gravitation of the particle to the whole spheroid will be proportional to the bigness of the spheroid and the difference betwixt the arch  $Cr$  (described on the center  $C$ ) and its tangent  $CF$ .

Again if  $M$  be a particle in the plain of the equator produced, it will gravitate to the part of the spheroid betwixt the two spheroidical surfaces with a force proportional to  $\frac{GL \times EK}{PC \sqrt{PC^2 - CF^2}}$ . And thence the gravitation of the particle to the whole spheroid will be found to depend on the quadrature of the circle, nay upon the forementioned difference  $CF$  and  $Cr$ . I have gone no further; but could accomplish what remains in a week or so if I had leisure. What I here send you are conclusions hastily drawn, and therefore I would not have them communicate because I have not yet examined them to my own satisfaction, and I write in such hast that I dont know if I have transcribed them right. I am in great hast

DS. Your most obedient humble Servant

JAMES STIRLING.

(14)

*Maclaurin to Stirling, 1738*

To

Mr James Stirling  
at Leadhills

Dear Sir

I believe you will find Mr Maitland usefull & exact and am glad he has so good an opportunity of improving himself under your eye. I wish you had time to finish what you are doing relating to the figure of the earth. I am informed that something is soon to be published on that subject at London by Celsius & others.

The account I gave you of some propositions had occurred to me on that subject was very imperfect. You may observe from what follows it, that when I spoke of concentric surfaces infinitely near I restricted it only that I might distinguish the parts more properly into such as were convex and concave towards the particle. I inquired into the ratio which I said was invariable & obtained it in a simple enough series which I have not reduced to the quadrature of the circle, tho' I conclude from your more perfect solution that it must be reducible to it. I did not try the problem by the concentric surfaces but in a different manner. And tho' I think your method must be better since an account of a different one may be agreable to you I shall describe the principal steps I took.

Supposing  $PB$  the shorter axis,  $AC$  the transverse semi-axis. I first computed the fluxion of the attraction of the solid generated by  $PMB$  while the figure revolves about the axis  $PB$ , and thence demonstrated what Mr Cotes says of the attraction of spheroids. By comparing what I had found in this with your account of the attraction of  $P$  I drew immediately on reading your letter this consequence that seems worthy of notice. That if  $PM$  meet a circle described from the center  $P$  with the radius  $PC$  in  $N$  and

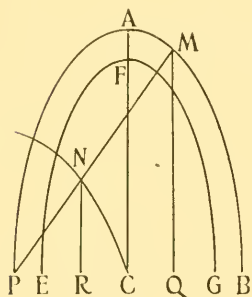


FIG. 11.

$NR$  be perpendicular to  $PB$  in  $R$ , &  $PE$  be taken equal to  $CR$ , and  $EFG$  be a similar concentric semiellipse, then the attraction of  $P$  towards the solid generated by  $EFG$  revolving about  $EG$  shall be equal to the attraction of  $P$  towards the solid generated by the segment  $PAM$  revolving about  $PB$ . This however I did not observe in the spheroid till I got your letter, in the sphere it is obvious.

After I had made out Mr Cotes's theorems, I then proceeded to consider the attraction at the equator, and still sought the fluxion of the attraction of the solid which seemed then to me to be more easily obtained than that of the concentric surfaces in this case especially. I supposed therefore the solid to be projected orthographically on the plane of the meridian  $PABD$ , the particle attracted I supposed to be directly over  $C$ ,

and to be in the pole of the meridian  $PABD$ ,  $NCM$  &  $nCm$  to be any two infinitely near ellipses passing through the particle; and then I computed the attraction of the matter included betwixt these two ellipses, or the fluxion of the attraction of the solid represented by  $CPM$ . Thus I found that if  $CP=a$ ,  $CA=b$ ,  $CF$  ( $F$  being the focus of the generating ellipse)  $=c$ , then the attraction of a particle at the equator towards the spheroid is to the attraction towards a sphere of the radius

$CA$  as  $\frac{a}{b} \times 1 + \frac{3c^2}{10b^2} + \frac{9c^4}{56b^4}$  &c: is to unit.

From this I computed the invariable ratio I mentioned in my last, wherein the difference of the tangent  $CF$  & ark  $CZ$  entered by Mr Cotes's theorem already spoke of.

But by your letter I perceive you have found the same invariable ratio without a series, by the quadrature of the circle only. From which I perceive that if the series I found be legitimate, as I cannot doubt but it is, it must be assignable by the circle. This perhaps would be easily found by examining it, but since you have done this already in effect I would willingly avoid the trouble. And only desyre you will let me know if the proportion given by this series agrees well enough with what you have found. I believe I might have computed your proportion from what you sent me, but there are so many of my acquaintance in town this week & I have had so little time that I have not got it done. I have some suspicion from the fluxion that gave this series that it is reducible to the circle, or to the square of it, by a way I have sometimes made use of and I believe is not new, of transforming a fluxion by the negative logarithms, but I have not made the computation necessary to judge of this.

You may be assured that I will communicate nothing of what you send me without your express *allowance*. I say something on this subject in my book, and would willingly add to it if you pleased, because since my book is grown to such a bulk I would willingly have as much new in it on the usefull problems as I can. I first proposed only to demon-

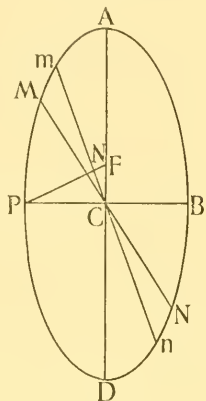


FIG. 12.

strate Mr Cotes's theorems in a brief manner enough after what Sir Isaac has on spheres, and so refer for the rest to your piece in the transactions; but I would think it more compleat to add this I have found since on the attraction at the equator & either subjoin that you had a more compleat solution which you would publish afterwards or mention, if you inclined that solution itself. In this I shall do just as you please.

I have not as yet tryed if the method I took for the attraction at the equator would succeed for computing the attraction at any other part of the spheroid, and hardly think it worth while to [ ] since you have a method that appears to be much better. All I have mentioned I did before I received your letter except the observation near [ ] end of the first page of this letter, else I had not taken so much p[ains] about it. I was chiefly induced to try it, because I imagined the method to be different from your's, and sometimes by following a different method conclusions come out more simple; but it has not proved so in this instance as far as I can judge of your r[esult].

I told you there were some fluxions which I had ground to suspect depended on the circle & hyperbola besides those described already by authors but I did not say that I had reduced these fluxions That I sent you is one of them, in certain cases of the variable quantity. I resolve to try it, but it is my misfortune to get only starts for minding those things & to be often interrupted in the midst of a pursuit. The enquiry, as you say, is rugged and laborious. This is the first post as I am told to the leadhills since I got your letter, and I shall be obliged to you if you will let me know without delay whether the series I described agrees with your solution by the circle which I imagine you will see at a look. I am

Dear Sir

Your Most Obedient

Dean May 20. 1738

Humble Servant

COLIN MACLAURIN

I have not the transaction by me where your paper is, else that perhaps would solve my question.

(15)

*Stirling to Maclaurin, 1738*

To

Mr Maclaurin    Professor of Mathematicks

in

Edenburgh

Leadhills    26 October 1738

D. S.

I was sorry that when I was last in Edinburgh I could not get time to wait on you. I got a letter this last summer from Mr Machin wholly relating to the figure of the Earth and the new mensuration, he seems to think this a proper time for me to publish my proposition on that Subject when everybody is making a Noise about it: but I chuse rather to stay till the French arrive from the South; which I hear will be very soon. And hitherto I have not been able to reconcile the measurement made in the north to the Theory: altho Dr Pound's and Mr Bradley's most accurate observations on the Diameters of Jupiter agree to two thirds of a second with my computation. Mr Machin tells me you write to him that you had hit on a demonstration to prove the figure of the earth to be a spheroid, on which I congratulate you, for my part hitherto I can only prove it by a computation.

I have lately had a letter from Mr Euler at Petersburgh, who I am glad to find is under no uneasiness about your having fallen on the same Theorem with him, because both his and the demonstration were publickly read in the Academy about four years ago; which makes me perfectly at quiet about it, for I was afraid of giving grounds of suspicion because I had long neglected to answer his first letter: his last one is full of a great many ingenious things, but it is long and I am not quite master of all the particulars. I have also heard lately from M. Clairaut, where he makes a great many apologies for having taken no notice of my paper about the figure of the earth when he sent his from Lapland to the Royal Society; and he tells me he has carried the matter further since that time in a new paper which he has also sent

to the Royal Society: now he says he has heard that I have been at some pains about that probleme and desires to have my opinion on his two papers. The first I barely saw before it was printed, and altho I had not time to read it thoroughly I soon saw that it was not of a low rank, as for the second I never saw it; and therefore I should be much obliged to you if you could favour me with a sight of both, that I might be able to answere his letter. If you can, please send them to Mr Maitland who will give them to Mr Charles Sherref at Leith with whom I correspond weekly, and they shall be carefully and speedily returned. I have yet had no time to meddle with that affair, and when I have, possibly I may not have inclination; but I shal be very glad to hear what you are doing & when we may expect to see your book

Sir

Your most obedient &  
most humble servant

JAMES STIRLING.

(16)

*Maclaurin to Stirling, 1740*

To

Mr James Stirling  
At Leadhills

Dear Sir

I designed to have writ last Saturday, but having gone to the country that forenoon, I did not get home that day. I am glad you are to send us a paper, and thank you for allowing Mr Maitland to come here for some days to help me to forward the plates. I will acquaint him when I shall be ready for him, that I may make that my only business (besides my Colleges) while he is here. We have some days of vacation about Christmas, if that time be not inconvenient for you I can find most leisure to apply to the figures then. I have so much drudgery in teaching, that I am commonly so fatigu'd at night I can do little business.

Mr Short writes that an unlucky accident has happened to the french Mathematicians in Peru. It seems they were

shewing some french gallantry to the natives wives, who have murdered their servants destroyed their Instruments & burn't their papers, the Gentlemen escaping narrowly themselves. What an ugly Article will this make in a journal

Mr Short saw the satellite of Venus Oct<sup>r</sup>. 23 for an hour in the morning, the phas is similar to that of Venus, but writes that he has never been able to see it since. His account agrees with Cassini's. It is a very shy planet it seems. Mr Graham has found that Brass has some influence on the magnetic needle, but I have not got a particular account of the experiments.

I wish I had an opportunity to shew you all that I have printed in my book relating to the attraction of spheroids and the figure of the Earth. In the mean time I shall give you some of the chief articles. 1. I begin with what I sent you two years ago, but the demonstration is somewhat different. 2. I give a general proposition concerning the attraction of a slice of a solid the figure of the section and position of the particle being given. 3. I apply this to spheres in a few words, and then to a spheroid.

The attraction at the pole is measured by an area easily reduced to the circle. The attraction at the equator by the complement of this area to a certain rectangle.

Here I take notice that you was the first that measured the attraction at the equator by a circle. 4. I easily reduce the attraction in the axis or equator produced to the attraction at the Pole and circumference of the equator, without any computation or new quadrature. 5. I apply this doctrine to the late observations & mensurations. 6. The result of this leads me to shew that a density increasing towards the center accounts for a greater increase of gravitation from the equator to the poles but not for a greater variation from the spherical figure; and that it is the contrary, when the density decreases towards the center. I then compute both in several hypotheses of a variable density, and then propose it as a query whether Dr Halley's hypothesis may not best account for the increase of gravitation & of the degrees at the same time. I afterwards treat of Jupiter, and find that supposing his density to increase with the depth uniformly so as to be 4 times greater at the center than at the surface, the mean of Dr Pound's ratios will

result. I find the variation from Kepler's law in the periods of his satellites arising from the spheroidal figure of the primary cannot be sensible. I shall send you the proposition you mention and would have sent it today, but I have been somewhat out of order. It would be better to send you the 2 or 3 sheets that relate to this subject if I could find a proper opportunity. I know not any particular reason for M<sup>r</sup> Machin's printing that piece of late. M<sup>r</sup> Short who engaged to send me the transactions has not as yet sent me M<sup>r</sup> Clairaut's 2<sup>d</sup> paper. I have printed all my book, excepting the 3 last sheets. The printers are very slow in the algebraic part, and I have little time at this season of the year. This with the figures will retard the publication I believe to the spring. I am

Dear Sir

Your Most Obedient

Humble Servant

COLIN MACLAURIN.

Edinburgh: Dec<sup>r</sup>. 6. 1740

## II

### SIR A. CUMING AND STIRLING

*Cuming to Stirling, 1728*

Kensington July 4<sup>th</sup> 1728

These were transmitted me from Scotland this day by  
M<sup>r</sup> George Campbell. I am

Dear M<sup>r</sup> Stirling

Your most obedient humble  
Servant

ALEX<sup>r</sup>. CUMING

Let water run out of y<sup>e</sup> circular hole *NBRD* whose radius  $BC = r$ . Let *AC* y<sup>e</sup> constant height of y<sup>e</sup> water above *C* y<sup>e</sup> center of y<sup>e</sup> hole be  $= a$ , and let  $Q =$  y<sup>e</sup> quantity of Water which wou'd be evacuated thro y<sup>e</sup> same hole in any given time  $t$ ; providing y<sup>e</sup> water was to run out at all parts of y<sup>e</sup> hole with y<sup>e</sup> celerity at y<sup>e</sup> center *C*. Then y<sup>e</sup> quantity of water which will be evacuated in y<sup>e</sup> same time will be =

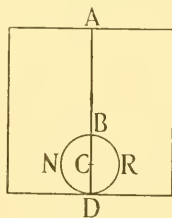


FIG. 13.

$$Q \times 1 - \frac{1}{2} \times \frac{1}{4} \times \frac{r^2}{a^2} + \frac{1}{4} \times \frac{3^2}{6} \times \frac{5}{8} \times \frac{r^4}{a^4} \\ + \frac{1}{4} \times \frac{3}{6} \times \frac{5^2}{8} \times \frac{7}{10} \times \frac{9}{12} \times \frac{r^6}{a^6} + \&c$$

Let *ADP* be y<sup>e</sup> elliptick Orbit which any of y<sup>e</sup> planets describes about y<sup>e</sup> Sun placed in one of y<sup>e</sup> foci *S*, let *F* be

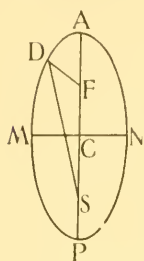


FIG. 14.

$y^e$  other focus,  $C$  its center,  $A$   $y^e$  aphelion,  $P$  the perihelion,  $SM$   $y^e$  mean distance of  $y^e$  planet from  $y^e$  sun, and let  $D$  be any place of  $y^e$  planet. Let  $SM$  or  $CA$  be  $= r$ ,  $y^e$  lesser semi Axe  $CM = c$ ,  $r - c = d$ , the excentricity  $SC = a$ , & let  $m$  represent  $y^e$  degrees in an arch of a circle equal to  $y^e$  radius or  $m = 57.29578$ . Let  $u$  be  $y^e$  sine of  $y^e$  angle  $AFD$ , and  $x$  the sine of its double  $y^e$  radius being  $= r$ . Then  $y^e$  difference between  $y^e$  angle  $AFD$  (which is  $y^e$  mean acquate anomaly) and  $y^e$  mean anomaly belonging to it, will be

$$\begin{aligned}
 &= \frac{2}{3} \frac{ma^3 u^3}{c^3 r^3} - \frac{4}{5} \frac{ma^5 u^5}{c^5 r^5} + \frac{6}{7} \frac{ma^7 u^7}{c^7 r^7} - \&c \\
 &\quad \pm \frac{md}{2r^2} \times 1 - \frac{9c^2 d + 8cd^2 + 2d^3}{3c^3} \times \frac{u^2}{r^2} + \\
 &\quad \frac{100c^3 d^2 + 145c^2 d^3 + 72cd^4 + 12d^5}{13c^5} \times \frac{u^4}{r^4} - \&c.
 \end{aligned}$$

From whence is deduced an easie method of determining  $y^e$  true anomaly from  $y^e$  mean anomaly being given.

Let the angle  $Y$  be found which beareth  $y^e$  same proportion to an angle of  $57.29578$  degrees which half  $y^e$  difference between  $y^e$  semi axes bears to  $y^e$  greater semi axe. Let also  $y^e$  angle  $Z$  be found bearing  $y^e$  same proportion to  $y^e$  angle of  $\frac{2}{3}$  of  $57.29578$  degrees or  $38.1971$  degrees which  $y^e$  cube of  $y^e$  excentricity bears to  $y^e$  cube of half  $y^e$  greater semi axe. Take an angle  $T$  proportional to  $y^e$  time in which the Arch  $AD$  is described or equal to  $y^e$  mean anomaly. Then let  $y^e$  angle  $V$  be to  $y^e$  angle  $Y$  as  $y^e$  sine of twice  $y^e$  angle  $T$  is to  $y^e$  radius, let also  $y^e$  angle  $X$  be to  $y^e$  angle  $Z$  as  $y^e$  cube of  $y^e$  sine of  $T$  is to  $y^e$  cube of  $y^e$  radius. then  $y^e$  mean acquat anomaly or  $AFD$  will be very near  $T + X + V$  when  $T$  is less than  $90^\circ$ , but  $T + X - V$  when  $T$  is more than  $90^\circ$  and less than  $180^\circ$ .

Let  $z$  represent  $y^e$  ratio of  $y^e$  centripetal force at  $y^e$  acuator of any planet to  $y^e$  power of gravity there, thus in  $y^e$  case of  $y^e$  Earth  $z = \frac{1}{289}$ . Then  $y^e$  aequatorial diameter will be to the Polar, as  $1$  is to  $1 - \frac{5}{4}z + \frac{5}{14}z^2 - \frac{545}{6272}z^3$  &c.

### III

## G. CRAMER AND STIRLING

(1)

*Cramer to Stirling, 1728*

To

Mr James Stirling F.R.S. in y<sup>e</sup> Academy  
in little Tower Street  
London

Sir,

Tis time to break off y<sup>e</sup> silence w<sup>ich</sup> I kept so long, tho' unwillingly. The wandering life of a traveller, and a long and tedious distemper, have been the only reason, why I did differ so long from giving you thanks for all the kindnesses and tokens of friendship you bestow'd upon me during my sojourn in London, and from making use of the permission you gave me of writing to ye, and inquiring into the littoral news of your country, but chiefly into the news of your health w<sup>ich</sup> is very dear to me.

The very day of my departure I received a Letter from M<sup>r</sup> Nicolas Bernoulli desiring me to present you his duties. In the same he demonstrates in an easy way, a General Principle whence it is not difficult to derive all y<sup>e</sup> Propositions of M<sup>r</sup> de Moivre about his *Seris recurrentes*. The principle is such. Let  $m+n+p+q$ , be the *Index* of y<sup>e</sup> Series, and inquire into y<sup>e</sup> Roots of y<sup>e</sup> Equation  $z^4 - mz^3 - nz^2 - pz - q = 0$  Let them be  $z, y, x, v$ : And make four Geometrical Series the Indices of whom be  $z, y, x, v$ . The Sum of y<sup>e</sup> respective Terms of these Geometrical Series is the respective Term of y<sup>e</sup> *Series recurrens* four terms of w<sup>ich</sup> may be given, because y<sup>e</sup> four first Terms of y<sup>e</sup> Geometrical Serieses are taken *ad*

*libitum* he demonstrates also his method for finding the Component quantities of a Binomium like  $1 \pm z^n$  by y<sup>e</sup> Division of ye Circle

I would fain know your opinion of this demonstration I found of M<sup>r</sup> de Moivre's first *Lemma* in his Doctrine of Chances. The Lemma is such

The number of chances for casting  $p+1$  points, with  $n$  Dices of  $f$  faces each is

$$\begin{aligned} & \frac{p \cdot \overline{p-1} \cdot \overline{p-2} \dots \overline{p-n+2}}{1 \cdot 2 \cdot 3 \dots \overline{n-1}} - \frac{n}{1} \times \frac{q \cdot \overline{q-1} \dots \overline{q-n+2}}{1 \cdot 2 \dots \overline{n-1}} \\ & + \frac{n(n-1)}{1 \cdot 2} \frac{r \cdot \overline{r-1} \dots \overline{r-n+2}}{1 \cdot 2 \dots \overline{n-1}} \\ & - \frac{n \cdot \overline{n-1} \cdot \overline{n-2}}{1 \cdot 2 \cdot 3} \times \frac{s \cdot \overline{s-1} \dots \overline{s-n+2}}{1 \cdot 2 \dots \overline{n-1}} \&c \begin{cases} q = p-f \\ r = q-f \\ s = r-f \end{cases} \\ & \&c. \end{aligned}$$

The Series is abrupted when one Term comes to be nought or negative.

My demonstration is grounded upon that principle that the number of chances for casting  $p+1$  points with  $n$  Dices is equal to the number of chances for casting  $p$  and  $p-1$  and  $p-2$  &c. to  $p-f+1 = q+1$  points with  $n-1$  Dices. For it follows that y<sup>e</sup> number of chances for casting  $p$  points with one Dice is  $p^0 - q^0$ , wich is equal to nought if  $q$  is positive that is if  $p$  is bigger than  $f$ , and equal to one if  $p =$  vel  $< f$ .

Now the number of chances for casting  $p+1$  points with two Dices is equal to y<sup>e</sup> number of chances for casting  $p$  with one Dice  $= p^0 - q^0$  + to y<sup>e</sup> number of chances for casting  $p-1$  with one Dice  $= \overline{p-1}^0 - \overline{q-1}^0$   
&c                      &c                      &c                      &c  
to y<sup>e</sup> number of chances for casting  $p-f+1$  with one Dice  $\overline{p-f+1}^0 - \overline{q-f+1}^0$

that is  $\overline{q+1}^0 - \overline{r+1}^0$

---

The Sum of y<sup>e</sup> 1<sup>st</sup> Col.  $p-q$   
of the 2<sup>d</sup> Col.  $-q+r$

---

Total sum  $p-2q+r$

I could proceed in the same manner to the case of three Dices, then to four, and so forth, and if I will, demonstrate in general that if the Lemma holds for the case of  $\overline{n-1}$  Dices it holds too for  $n$  Dices.

Mr S. Gravesande, who is wholly employ'd about y<sup>e</sup> Doctrine of forces, did communicate me the following construction for the laws of percussion.

Let  $A$  and  $B$  be two bodies Elastic or not Elastic.  $AL, BL$  their respective velocities before the shock. Let  $D$  be their

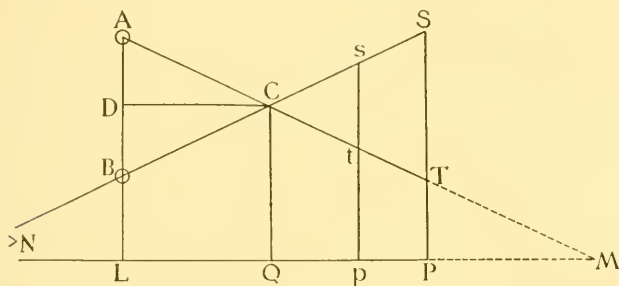


FIG. 15.

center of gravity, and  $DC$  be drawn perpendicular to  $AB$  of an indeterminate length. Draw  $AC$ ,  $BC$  to be prolong'd if it needs.

Now if the bodies are not Elastic,  $QC$  will be the common velocity after  $y^e$  percussion. If they are Elastic, take  $Cs = CB$  and  $CT = CA$  and  $PT$  shall be the velocity of  $y^e$  Body  $A$ , and  $PS$  the velocity of the Body  $B$  after  $y^e$  Concussion.

· If they are imperfectly Elastic, take  $Cs$  to  $CS$  and  $Ct$  to  $CT$  as  $y^o$  elasticity to the perfect elasticity and  $Ct$ ,  $Cs$  shall be the velocitys of the Bodies  $A$  and  $B$ . In his opinion about the forces of the Bodies, this construction is very commodious, for before the percussion  $ALM$  represents the force of  $y^o$  Body  $A$ , and  $BLN$  the force of  $y^o$  Body  $B$ . But after  $y^o$  percussion  $CTM$  and  $CN$  are the forces of the bodies  $A$  and  $B$ , if they are elastic, and  $C'QM$   $C'QN$  are these forces if they are not elastic, and  $ACB$  is the force lost in  $y^o$  percussion

Mr 'S Gravesande demonstrates it, by this proposition, That y<sup>e</sup> instantaneous mutations of forces in the two bodies, are proportional to their respective velocities. But I found that

it could be proved, without the new notion of forces, by this proposition. That y<sup>e</sup> contemporaneous mutations of velocities of the two bodies are reciprocal to their masses wick can be evin'd in several manners, and very easily, if granted that the common center of gravity does not alter its velocity by the percussion.

I am just arrived at Paris, and so have no news from france to impart with ye. You'll oblige me very much, if you vouchsafe to answer to this, and inform me about your occupation and those of your Royal Society and its learned members. Did M<sup>r</sup> Machin publish his Treatise about y<sup>e</sup> Theory of y<sup>e</sup> Moon? Is M<sup>r</sup> de Moivre's Book ready to be published? Is there nothing under the press of S<sup>r</sup> Isaac's remains? What are you about? Can we flatter ourselves of the hopes of seeing very soon your learned work about y<sup>e</sup> Series? All these and other news of that kind, if there are some, will be very acceptable to me; and I'll neglect nothing for being able of returning you the like, as much as the sterility of the country I live in, and my own incapacity will allow. In the meanwhile, I desire you to be fully persuaded, I am, with all esteem and consideration

Sir

Your most humble

Most obedient Servant

Paris, this  $\frac{11}{22}$  X<sup>bro</sup> 1728

G. CRAMER

You can direct y<sup>e</sup> Answer

A Messieurs Rilliet & Delavine, rue Grenier S<sup>t</sup> Lazare pour rendre à M<sup>r</sup> Cramer à Paris.

(2)

*Cramer to Stirling, 1729*

To

M<sup>r</sup> James Stirling F R.S. at the  
Academy in little Tower Street  
London

Here is, Dear Sir, a Letter from M<sup>r</sup> Nich. Bernoulli in answer to yours, wick I received but t'other day. I send with it,

according to his Orders a Copy of his method of resolving y<sup>o</sup> quantity  $\frac{1}{1 \pm qz^n + z^{2n}}$  in its component fractions the former part of wick he sent me to Paris, by M<sup>r</sup> Klingenstiern the supplement I had but in the same time with your Letter. I hope you have lately received from me an answer to your kind Letter brought by M<sup>r</sup> Sinclair. I am with a great esteem

Your most humble  
and obedient Servant

Geneva the 6<sup>th</sup> January, 1729. N.S.

G. CRAMER.

Methodus resolvendi quantitates  $1 \pm qz^n + z^{2n}$  in factores duarum Dimensionum, Auctore D<sup>o</sup>. Nicolao Bernoulli.

Prob. I Resolvere quantitatem  $1 \pm qz^n + z^{2n}$  in factores duarum Dimensionum.

Solut. Sit unus ex factoribus  $1 - xz + zz$

& productum reliquorum

$$1 + az + bz^2 + cz^3 \dots + rz^{n-3} + sz^{n-2} + tz^{n-1} + sz^n + rz^{n+1} \dots \\ + bz^{2n-4} + az^{2n-3} + z^{2n-2}.$$

Ex comparatione terminorum homogeneorum producti horum factorum cum terminis propositae quantitatis invenitur  $a = x$ ,  $b = ax - 1$ ,  $c = bx - a$  & ita porrho usque ad  $t = sx - r$ , item  $\pm q = 2s - tx$ , adeo ut quantitates  $1, a, b, c, \dots r, s, t$  constituent Seriem recurrentem in quâ quilibet terminus per  $x$  multiplicatus est aequalis Summae praecedentis & sequentis. Jam vero si Chorda complementi  $BD$  alicujus arcus  $AD$  vocetur  $x$  & radius  $AC = 1$  Chordae arcuum multorum ejusdem arcus  $AD$  exprimentur respectivè per eosdem terminos inventae Seriei recurrentis  $1, a, b, c$ , &c. multiplicatos per Chordam  $AD$ . Hinc si arcus  $AE$  sit ad arcum  $AD$  ut  $n$  ad 1, erit Chorda  $AE$  ad Chordam  $AD$  ut  $t$  ad 1, id est  $AE = t \times AD$ , & Chorda  $DE = s \times AD$ . Ex natura vero quadrilateri  $ADEB$

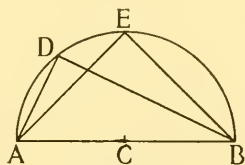


FIG. 16.

circulo inscripti est  $AE$ .  $DB = AB \cdot DE + AD \cdot BE$  id est

$$tx \cdot AD = 2s \cdot AD + AD \cdot BE$$

sive  $tx = 2s + BE = (\text{quia } \pm q = 2s - tx) tx \pm q + BE$ ,

$$\text{hinc} \quad BE = \mp q.$$

Ex quo sequitur quod si arcus habens pro Chorda complementi  $\mp q$  dividatur in  $n$  partes aequales quarum una sit arcus  $AD$ , hujus complementi Chorda futura sit  $x$ : vel si rem per Sinus conficere malimus, dividendus est arcus habens pro Cosinu  $\mp \frac{1}{2}q$  in  $n$  partes aequales, qui si vocetur  $A$ , erit cosinus arcus  $\frac{A}{n} = \frac{1}{2}x$ . Invento valore ipsius  $x$  cognoscitur  $1 - xz + z^2$  unus ex factoribus quantitatis propositae  $1 \pm qz^n + z^{2n}$ . Sed & reliqui factores hinc cognoscuntur. Si enim tota circumferentia vocetur  $C$ , habebunt omnes sequentes arcus  $A$ ,  $C - A$ ,  $C + A$ ,  $2C - A$ ,  $2C + A$ ,  $3C - A$ ,  $3C + A$ , &c pro Cosinu  $\mp \frac{1}{2}q$ , quorum singuli in partes aequales divisi determinabunt totidem diversos valores ipsius  $x$ .

Coroll. 1. Per methodum serierum recurrentium invenitur  $x =$  radici hujus aequationis

$$\sqrt{qq-4} = (\tfrac{1}{2}x + \sqrt{\tfrac{1}{4}xx-1})^n - (\tfrac{1}{2}x - \sqrt{\tfrac{1}{4}xx-1})^n$$

Coroll 2. Si capiatur arcus  $AH$  aequalis alicui sequentium arcuum  $\frac{A}{n}$ ,  $\frac{C-A}{n}$ ,  $\frac{C+A}{n}$ ,  $\frac{2C-A}{n}$ ,  $\frac{2C+A}{n}$  &c & fuerit

$CG = z$  erit  $GH =$  radici quadratae factoris  $1 - xz + z^2$ . Quia enim  $CF = \frac{1}{2}x$  erit  $GF = \frac{1}{2}x - z$ ,  $FH = \sqrt{1 - \frac{1}{4}x^2}$  & proinde  $GH = \sqrt{1 - xz + z^2}$ .

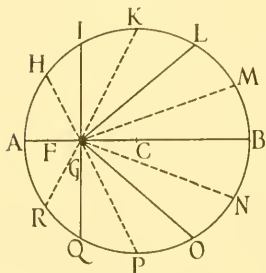


FIG. 17.

Coroll 3. Si  $q = 0$ , erit  $A = \frac{1}{4}C$ , & reliqui arcus dividendi  $\frac{3}{4}C$ ,  $\frac{5}{4}C$ ,  $\frac{7}{4}C$ ,  $\frac{9}{4}C$  &c. Hinc si dividatur tota circumferentia in  $4n$  partes aequales  $AH$ ,  $HI$ ,  $IK$ , &c & ad singulos impares divisionis terminos  $H$ ,  $K$ ,  $M$ , &c. ex puncto  $G$  ducantur rectae  $GH$ ,

$GK$ , &c erit horum omnium productum  $1 + z^{2n}$ .

## Probl. II

Resolvete quantitatem  $1 + z^{2n+1}$  in factores duarum Dimensionum.

Solut. Sit unus ex factoribus  $1 - xz + zz$ , & productum reliquorum

$$1 + az + bz^2 + cz^3 \dots rz^{n-3} + sz^{n-2} + tz^{n-1} + tz^n + sz^{n+1} + rz^{n+2} \dots \\ + bz^{2n-3} + az^{2n-2} + z^{2n-1}.$$

& invenitur ut antea  $a = x$ ,  $b = ax - 1$ ,  $c = bx - a$ , & ita porrho usque ad  $t = sx - r$ . Sed loco aequationis  $\pm q = 2s - tx$  invenietur haec  $t = tx + s = 0$  id est, si ponatur arcus  $AD$  ad arcum  $AE$ , ut 1 ad  $n$ , erit (quia  $t = \frac{AE}{AD}$  &  $s = \frac{DE}{AD}$ , &  $x = BD$ )  $AE - AE \cdot BD + DE = 0$ . Sive  $DE = AE \cdot BD - AE$  & aequatione in analogiam versa

$$DE : AE = BD - 1 : 1 = (\text{facta } DF = DC = AC = 1) BF : CB.$$

Hinc triangula  $ADE$ ,  $CFB$ , ob angulos ad  $E$  &  $B$  aequales, erunt similia & angulus  $BCF = DAE$ .

Ergo ang.  $BCF + \text{ang } CBF = \text{ang } DFC = \text{ang } DCF = \text{ang } DAE + \text{ang } CBF$  Sed &  $\text{ang } CDF = \text{ang } CBF$ . Hinc omnes tres anguli Trianguli  $CDF$  sunt aequales 2 ang.  $DAE + 3 \text{ ang } CBF$  ipsorum que mensura, id est, semicircumferentia

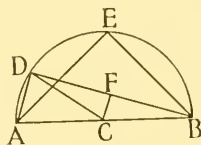


FIG. 18.

$$= \frac{1}{2}C = \text{arc } DE + \frac{3}{2} \text{ arc } AD$$

$$= (\text{quia arc } DE = n - 1 \text{ arc } AD) \frac{2n + 1}{2} \text{ arc } AD.$$

ideoque arcus  $AD = \frac{C}{2n + 1}$ . Si igitur circumferentia Circuli

dividatur in  $2n + 1$  partes aequales, quarum una sit arcus  $AD$ , erit Chorda  $BD = x$ , vel si semicircumferentia in totidem partes aequales dividatur, erit cosinus unius partis  $\frac{1}{2}x$  unde cognoscetur factor  $1 - xz + zz$ . Quia vero tot factores duarum dimensionum inveniendi sunt quot unitates sunt in numero  $n$ , habebit totidem diversos valores qui erunt dupli cosinus 1, 3, 5, 7 &c partium semicircumferentiae in  $2n + 1$  partes aequales divisae: invenitur enim arcus  $AD =$  singulis sequentibus

$$\text{arcubus } \frac{C}{2n + 1}, \frac{3C}{2n + 1}, \frac{5C}{2n + 1}, \frac{7C}{2n + 1}, \text{ &c, quia arcus } AE$$

qui est ad arcum  $AD$ , ut  $n$  ad 1. potest intelligi auctus integra Circumferentia vel ejus multiplo, hoc modo igitur resolvetur quantitas proposita  $1 + z^{2n+1}$  in  $n$  factores duarum dimensionum & unum factorem  $1 + z$  unius dimensionis.

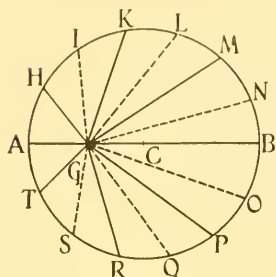


FIG. 19.

Coroll. Si fuerit

$$CG = z, \quad AC = CB = 1$$

& Circumferentia circuli dividatur in  $4n + 2$  partes aequales  $AH, HI, IK, \&c$  ad singulos impares divisionis terminos  $H, K, M, \&c$  ducantur rectae

$GH, GK, GM, \&c$ , erit horum omnium productum aequale  $1 + z^{2n+1}$ .

Probl. III Resolvere quantitatem  $1 - z^{2n+1}$  in factores duarum Dimensionum.

Solut. Sit unus ex factoribus  $1 - xz + z^2$  & productum reliquorum

$$1 + az + bz^2 + \dots + rz^{n-3} + sz^{n-2} + tz^{n-1} - tz^n - sz^{n+1} - rz^{n+2} \dots - bz^{2n-3} - az^{2n-2} - z^{2n-1}$$

& invenietur  $s = t + tx$ : reliqua vero se habent ut prius. Positis igitur ut in Prob II arcu  $ADBE = n$  arc  $AD$ ,  $x = BD$ ,

$$t = \frac{AE}{AD}, \quad s = \frac{DE}{AD}, \quad \text{erit } DE = AE + AE \cdot BD.$$

Hinc  $DE : AE = BD + 1 : 1 = (\text{facta } DF = DC = 1) BF : CB$

Proinde triangula  $ADE, CFB$  habentia angulos ad  $E$  &  $B$  aequales erunt similia, & ang  $BCF = \text{ang } DAE$ : quamobrem ang:  $F = \text{ang: } DCF = \text{ang } BCF - \text{ang: } BCD = \text{ang: } DAE - \text{ang: } BCD$ . Hinc omnes tres anguli trianguli  $BCF$  sunt = ang:  $B + 2 \text{ ang: } DAE - \text{ang: } BCD$ : ipsorum que mensura

$$\frac{1}{2}C = \frac{1}{2} \text{arc: } AD + \text{arc: } DBE - \text{arc: } BD = \frac{1}{2} \text{arc: } AD + \text{arc: } BE = \overline{n + \frac{1}{2} \text{arc: } AD} - \frac{1}{2}C.$$

$$\text{Hinc } C = \overline{n + \frac{1}{2} \text{arc } AD}, \quad \& \quad \text{arc } AD = \frac{C}{n + \frac{1}{2}} = \frac{2C}{2n + 1},$$

ejus dimidii, nempe  $\frac{C}{2n + 1}$ , cosinus erit  $\frac{1}{2}x$ . Si arcus  $ADBE$

intelligatur auctus integrâ circumferentiâ vel ejus multiplo invenientur reliqui valores ipsius  $\frac{1}{2}x$  aequales cosinibus arcuum

$$\frac{2C}{2n+1}, \frac{3C}{2n+1}, \frac{4C}{2n+1}, \text{ \&c. Et}$$

sic resolvetur quantitas proposita  $1 - z^{2n+1}$  in  $n$  factores duarum Dimensionum, & unum factorem  $1 - z$  unius dimensionis.

Coroll. Si in fig. Coroll. praeced. ad singulos pares terminos  $I, L, N, O$  &c. ducantur rectae  $GI, GL, GN, GO$ , &c. erit harum omnium productum  $= 1 - z^{2n+1}$ .

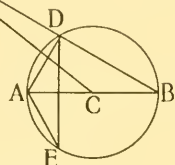


FIG. 20.

Probl IV Resolvere quantitatem  $1 - z^{2n}$  in factores duarum Dimensionum.

Solut. Sit unus ex factoribus  $1 - xz + zz$  & productum reliquorum

$$1 + az + bz^2 \dots + rz^{n-3} + sz^{n-2} \pm tz^{n-1} - sz^n - rz^{n+1} \dots - bz^{2n-4} \\ - az^{2n-3} - z^{2n-2}.$$

Hic quia terminus  $tz^{n-1}$  debet affici signo tam affirmativo quam negativo, oportet esse  $t = 0$ , adeoque si ponatur arcus  $AD$  ad arcum  $AE$  ut 1 ad  $n$ , & per consequens  $t = \frac{AE}{AD}$ , erit  $AE = 0$ , & arcus huic Chordae respondens  $=$  vel  $C$ , vel  $2C$ , vel  $3C$  &c. Proinde arcus  $AE =$  alicui sequentium arcuum  $\frac{C}{n}, \frac{2C}{n}, \frac{3C}{n}$ , &c. &  $\frac{1}{2}x =$  cosinibus arcuum  $\frac{C}{2n}, \frac{2C}{2n}, \frac{3C}{2n}$ , &c qua ratione resolvitur quantitas  $1 - z^{2n}$  in  $n-1$  factores duarum Dimensionum similes huic  $1 - xz + zz$ , & alium factorem duarum dimensionum, nempe  $1 - zz$ .

Coroll. Si in fig. Cor. 2 & 3, Probl 1 ad singulos pares terminos divisionis  $I, L, B, O, Q, A$ , Ducantur rectae  $GI, GL, GB$  &c, erit harum omnium productum  $= 1 - z^{2n}$ .

Coroll. generale. Si Circumferentia Circuli dividatur in  $2m$  partes aequales  $AH, HI, IK$ , &c, & ducantur rectae  $GH, GI, GK$  &c sive  $m$  sit numerus par, sive impar semper erit  $GH \times GK \times GM$  &c  $= 1 + z^m$ , &  $GA \times GI \times GL$  &c  $= 1 - z^m$ .

Quod est Theorema Cotesii memoratum

Act. Erud. Lips. 1723, pag. 170 & 171.

## Supplementum Eodem Auctore

Probl. V Dividere fractionem  $\frac{1}{1 \pm qz^n + z^{2n}}$  in fractiones plures, quarum denominatores ascendant tantum ad duas Dimensiones.

Solut. Sit una quaesitarum fractionum  $\frac{e-fz}{1-xz+zz}$  & summa reliquarum  $\frac{\alpha + \beta z + \gamma zz + \delta z^3 + \epsilon z^4 + \&c}{1 + az + bz^2 + cz^3 + dz^4 + \&c}$

Valor ipsius  $x$  determinatur in Problemate primo, & quantitates  $1, a, b, c, d, \&c$  designant ut ibidem terminos Series recurrentis  $1, x, xx-1, x^3-2x, x^4-3xx+1, \&c$ . Valores autem ipsarum  $e$  &  $f$  post eliminationem ipsarum  $\alpha, \beta, \gamma, \delta$  &c inveniuntur ut sequitur: nempe si  $n = 2$ , id est si

$$\frac{1}{1 \pm qz^2 + z^4} = \frac{e-fz}{1-xz+zz} + \frac{\alpha + \beta z}{1+xz+zz},$$

invenitur  $e = \frac{1}{2}$  &  $f = \frac{1}{2} \times \frac{1}{x}$  Si  $n = 3$ , id est si

$$\frac{1}{1 \pm qz^3 + z^6} = \frac{e-fz}{1-xz+zz} + \frac{\alpha + \beta z + \gamma zz + \delta z^3}{1+xz+xx-1zz+xx^3+z^4}$$

invenitur  $e = \frac{1}{3}$ , &  $f = \frac{1}{3} \frac{x}{xx-1}$ : si  $n = 4$ , id est si

$$\frac{1}{1 \pm qz + z^8} = \frac{e-fz}{1-xz+zz} + \frac{\alpha + \beta z + \gamma zz + \delta z^3 + \epsilon z^4 + \zeta z^5}{1+xz+xx-1zz+xx^3-2xz^3+xx-1z^4+xx^5+z^6}$$

invenitur  $e = \frac{1}{4}$  &  $f = \frac{1}{4} \frac{xx-1}{x^3-2x}$ : similiter si  $n = 5$

invenitur  $e = \frac{1}{5}$  &  $f = \frac{1}{5} \frac{x^3-2x}{x^4-3xx+1}$ , & generaliter ob ratio-

nem progressionis jam satis manifestam erit  $e = \frac{1}{n}$  et  $f = \frac{1}{n} \frac{s}{t}$ , ubi  $s$  &  $t$  significant duos postremos terminos Series recurrentis  $1, a, b, c, d, \&c$ . Hinc si in fig. Probl. I sit Chorda  $BE = +q$

& arcus  $AD = \frac{\text{arc } AE}{n}$ , erit  $s:t = DE:AE$  per ibi demonstrata,

& per consequens  $f' = \frac{DE}{n \cdot AE}$ , ipsaque quaesita fractio

$$\frac{e-fz}{1-xz+zz} = \frac{1}{n} - \frac{DE}{n \cdot AE} z.$$

Si porrho intelligatur arcus  $AE$  auctus integra circumferentia vel ejus multiplo, ita ut mutantur valores ipsarum  $BD$  &  $DE$ , mutabitur quoque valor fractionis  $\frac{e-fz}{1-xz+zz}$  invenienturque successive omnes fractiones in quas proposita fractio  $\frac{1}{1 \pm qz^n + z^{2n}}$  resolvi potest Q.E.F.

Coroll. Si  $q = 0$ ,  $DE = DB = x$ ,  $AE = AB = 2$ , fractio  $\frac{1}{1+z^{2n}}$  resolvitur in fractiones hanc formam  $\frac{1}{n} - \frac{x}{2n} z$  habentes.

Schol I Solutio inventa congruit cum ea quam Pemberton ex calculo valde operoso deduxit in Epist. ad amicum pag. 48 & 49 & ejus appendice pag. 11, 12. Est quoque simplicior quam Moivraei qui invenit fractiones hanc formam habentes

$\frac{1}{n} - \frac{a-le}{n-un} z$  ubi  $a = \frac{1}{2}x = \sinui \frac{1}{2}$  arcus  $BD$ ,  $l = \mp \frac{1}{2}q = \sinui$

$\frac{1}{2}$  arcus  $BE$ ,  $e = \cosinui \frac{1}{2}$  arcus  $DE$ , potuisset enim adhibere

hanc simpliciozem expressionem  $\frac{1}{n} - \frac{ez}{n\sqrt{1-u}}$  intelligendo per

$e$  non cosinum sed ipsum sinum  $\frac{1}{2}$  arcus  $DE$

Schol II Non absimili methodo resolvi possunt fractiones

$$\frac{1}{1 \pm z^{2n-1}} \quad \text{vel} \quad \frac{1}{1 - z^{2n}}.$$

Schol III Methodus praeced. supponit  $q$  minorem binario, quando autem  $q > 2$ , fractio  $\frac{1}{1 \pm qz^n + z^{2n}}$  resolvi potest ut

ostendit Moivraeus, in duas has  $\frac{\alpha}{1 \pm x^n} + \frac{\beta}{1 \pm y^n}$  ponendo

$$x^n = z^n \times \frac{1}{2}q + \sqrt{\frac{1}{4}qq - 1} \quad \& \quad y^n = z^n \times \frac{1}{2}q - \sqrt{\frac{1}{4}qq - 1},$$

$$\alpha = \frac{1}{2} + \frac{q}{2\sqrt{qq-1}} \quad \text{and} \quad \beta = \frac{1}{2} - \frac{q}{2\sqrt{qq-1}}.$$

Schol. IV Sint  $x, x^i, x^{ii}, x^{iii}, x^{iv}$  &c valores omnes ipsius  $x$  seu radices hujus aequationis,

$$\sqrt{qq-4} = \frac{1}{2}x + \sqrt{\frac{1}{4}xx-1}^n - \frac{1}{2}x - \sqrt{\frac{1}{4}xx-1}^n$$

vel potius hujus

$$\pm q = \frac{1}{2}x + \sqrt{\frac{1}{4}xx-1}^n + \frac{1}{2}x - \sqrt{\frac{1}{4}xx-1}^n$$

in Coroll. Prob I inventae, et significant  $e, f, s, t$ , idem quod supra, per ea quae Pemberton non sine magno labore invenit in Epist. pag. 49. est

$$e = \frac{t}{x-x^i, x-x^{ii}, x-x^{iii} \quad \&c} \quad \& \quad f = \frac{s}{x-x^i, x-x^{ii}, x-x^{iii} \quad \&c}$$

Denominator harum fractionum invenitur per Regulam Moivraei dividendo differentialem quantitatis

$$\left| \frac{1}{2}x + \sqrt{\frac{1}{4}xx-1} \right|^n + \left| \frac{1}{2}x - \sqrt{\frac{1}{4}xx-1} \right|^n$$

per  $dx$  & habebimus

$$\begin{aligned} & \overline{x-x^i, x-x^{ii}, x-x^{iii} \quad \&c} \\ &= \frac{n \cdot \left| \frac{1}{2}x + \sqrt{\frac{1}{4}xx-1} \right|^n - n \left| \frac{1}{2}x - \sqrt{\frac{1}{4}xx-1} \right|^n}{2\sqrt{\frac{1}{4}xx-1}} \end{aligned}$$

= (per methodum Serierum recurrentium)  $nt$ .

$$\text{Hinc} \quad e = \frac{t}{nt} = \frac{1}{n}, \quad \& \quad f = \frac{s}{nt} \text{ ut supra.}$$

Schol. V Ut Regula Moivraei quae facillime deducitur ex art. 163 de l'Analyse des Infinim petits possit applicari, oportet aequationem esse *debite* praeparatam, id est, ita comparatam ut nulla mutatione, multiplicatione vel divisione opus sit ad inveniendum terminum pure cognitum, qui prodit quando

Radix ab omni vinculo liberatur & terminus altissimae dignitatis nullo coefficiente afficitur ut contingit in ista aequatione

$$\pm q = \overline{\frac{1}{2}x + \sqrt{\frac{1}{4}xx - 1}}^n + \overline{\frac{1}{2}x - \sqrt{\frac{1}{4}xx - 1}}^n$$

non autem in altera

$$\overline{\frac{1}{2}x + \sqrt{\frac{1}{4}xx - 1}}^n - \overline{\frac{1}{2}x - \sqrt{\frac{1}{4}xx - 1}}^n = \sqrt{qq - 4}.$$

(3)

*Cramer to Stirling, 1729*

Viro Clarissimo, Doctissimo

Jacobo Stirling

L.A.M. & R.S. Socio

Gabriel Cramer

S.P.D.

Dominum Klingnestierna Matheseos Professorem Vpsaliensem amicum meum intimum eo digniorem esse familiaritate tua intelliges, quo tibi intimius innotescet.

Is cum apud Germanos haud vulgaris Mathematici famam reportasset & a Joh. Bernoullio mihi magnopere commendatus mecum Parisiis degeret: in Angliam profecturus est ut Mathematicorum tuique in primis consuetudine uteretur. Ubi tuum in me amorem intellexit, confidit his meis literis se apud te gratiosum fore quae ne spes eum fallat vehementer rogo te: Sed ut ad eam voluntatem quam tua sponte erga ipsum habiturus esses, tantus cumulus accedat commendatione mea, quanti me à te fieri intelligo. Hoc mihi gratius facere nihil potes. Vale.

Dabam Genevae ad diem 20 Junii 1729.

Mr James Stirling F.R.S. at y<sup>e</sup> Academy

in little Tower Street

London.

(4)

*Cramer to Stirling, 1729*

Mr James Stirling at the Academy  
in little Tower Street  
London

Sir

I received some days ago your dear letter, wick in such a Town, and such a Time of Carnaval, I could not find any proper moment to answer sooner. I wrote this morning to Mr Nich. Bernoulli and presented him your compliments. I gave him advise too of your Mind of writing to him. As for his direction, if you will be so kind as to permit me to be the Mediator of that correspondence I'll be infinitely obliged to ye: and you ought but to send me the Letter, wick shall arrive safe to him.

I don't know whether he has thought upon that difficulty wick you made me advert to; of finding any term whatsoever of a Series recurrens. when  $y^e$  Divisor by wick it is produced being put equal to nought, has impossible roots: but I found an easy way of determining it by  $y^e$  help of Tables of Sines already calculatèd. For it is known that each equation wick has impossible roots, has an even number of them and consequently may be reduced to as many quadratick equations as many couples of impossible roots it has: therefore  $y^e$  fraction by  $y^e$  division of wick  $y^e$  Series is produced may be reduced to as many fractions whose denominator shall be quadratick; besides, perhaps, some others whose denominator is simple. Let the fraction whose denominator is quadratick be represented by that general expression  $\frac{1}{1+mx+nx^2}$  where, in  $y^e$  case of two impossible roots  $n$  is positive and  $mm$  less than  $4n$ . Now in order to find any term whatsoever of the Series produced by that fraction for inst.  $y^e$  term  $l^{th}$  in order. Let  $\sqrt{n}$  be  $y^e$  Radius of a Circle, and  $\frac{m}{2}$  be  $y^e$  Cosine of an Arch  $z$  of that Circle: take the Sine of  $y^e$  Arch  $z$ , multiply it by  $n^{\frac{l-1}{2}}$ , and divide it by  $y^e$  Sine of  $y^e$  Arch  $z$ . The quotient

will be  $y^e$  Term required. The Demonstration follows easily from that Observation, that 1 being the first term; and the sine of an Arch  $z$   $y^e$  second term of a Series recurs, whose index is  $2c - rr$  ( $c$  being  $y^e$  cosine of  $y^e$  arch  $z$ , and  $r$   $y^e$  radius) each term  $l$  is equal to  $y^e$  Sine of  $y^e$  Arch  $lz$  multiplied by  $y^e$   $l-1$  power of  $y^e$  radius. Where 'tis to be observed, that if  $m$  be positive, you needs but to render all  $y^e$  even Terms negatives.

I am glad that Mr de Moivre's Lemma is by me demonstrated in a manner that pleases ye; and since you have seen Mr De Moivre's own demonstration, I am anxious to know how far it agrees or differs from mine.

I'll see with a great pleasure M. Maclaurin's Book about vivid forces, but I fear it shall pass a long time before it comes into my hands, because English books come abroad very late: unless you wou'd be so good as to procure one to Mr Caille where I did lodge in Aldermary Churchyard. he shou'd pay for it, and find some way of sending it to me here in Paris. I'll be very obliged to ye for that trouble, and will be very glad to render ye any Services, when you'll judge fit to command.

Shall Mr Bradley's account of  $y^e$  newly observed motion of  $y^e$  fixt Stars appear in  $y^e$  Philosophical transactions, or by itself? If so, I desire you to take the same trouble about it, as about Mr Maclaurin's book.

I long after seeing your book about Series, and intreat you not to put off  $y^e$  printing of it, being sure that whatever set forth from your hands is excellent, and will be very welcome in Publick.

I desire you to be so kind as to give me advice, when Mr de Moivre's book shall be published, because Mr Caille has got a Subscription for me, and I'll be glad to peruse  $y^e$  book as soon as it shall be publish'd.

A learned friend of mine, Mr de Mairan, I should much oblige, if I cou'd by your help, give him an account of a Letter wich Dr Halley wrote about twenty years ago, to Mr Maraldy, in answer to a Discourse, wich this printed in  $y^e$  French Academy's Memoirs A° 1707. against  $y^e$  commonly received opinion of  $y^e$  Successive propagation of Light: wherein he endeavours to argue against Mr Roemer's and Sr Isaac Newton's

demonstration drawn from y<sup>e</sup> Observations of y<sup>e</sup> Satellites of Jupiter's Emersions and Immersions. M<sup>r</sup> de Mairan wishes to know, in what time exactly y<sup>e</sup> Letter was written, and its contents. If you cou'd help me to a copy of it, or, at least, to a short abstract of what is most material in it, I shou'd think myself infinitely oblidged t'ye.

I am ashamed to trouble ye with so much business, but I hope your friendship will excuse me, and that in like cases, you will be not sparing of my trouble, wich I shall very willingly take, being with a great esteem and a sincere affection,

Your most humble and  
obedient Servant

G. CRAMER

Paris y<sup>e</sup> 12<sup>e</sup> March 1729. N.S.

(5)

*Cramer to Stirling, 1729*

To

Mr James Stirling, F.R.S. at the Academy  
in little Tower Street  
London

Geneva, y<sup>e</sup>  $\frac{19}{30}$  May 1729.

Sir

The place whence I date this Letter, will be, I hope, a sufficient excuse for having been so long in your Debt. I return you my humble thanks for all the trouble you took on my occasion, and shou'd think myself happy to find some opportunity of doing you any Service. I received, since y<sup>e</sup> last time, I wrote ye, a Letter from M<sup>r</sup> Nicolas Bernoulli who seems to be very glad of your correspondence and expects your Letters impatiently. My direction is now, *A Monsieur Cramer, Professeur en Mathematique à Genève*. You may spare y<sup>e</sup> trouble of freeing them, from London to Paris, if you'll wrap them in a sheet of Paper directed, *A Monsieur le Fevre Commis de la Poste, a Paris*.

I grant ye, my way of assigning a Term of a Recurring Series, when y<sup>e</sup> Denominator of y<sup>e</sup> Fraction hath impossible

Roots is not general enough: for I thought not of y<sup>e</sup> Case you make mention of: but I doubt very much of y<sup>e</sup> Possibility of a general Solution, for it seems to include a General Solution of any Equation.

I have seen lately a Dissertation that M<sup>r</sup> Daniel Bernoulli, M<sup>r</sup> John Bernoulli's son, did read in y<sup>e</sup> Petersburg's Academy concerning the recurring Serieses. What seem'd to me most material and, I believe, new is that he deduces from this Serieses, an easy and elegant way of founding by approximation two Roots of any Equation, viz: the greatest and y<sup>e</sup> smallest. The Method is such.

Let the Equation be disposed after this form—

$$1 = ax + bx^2 + cx^3 + \&c,$$

and make a recurring Series beginning by as much arbitrary Terms as dimensions The Equation has, and y<sup>e</sup> index of y<sup>e</sup> Series be  $a + b + c + \&c$ : and any Term divided by y<sup>e</sup> subsequent shall be equal or very near to y<sup>e</sup> Smallest root. The greatest root is found in y<sup>e</sup> same manner if this is y<sup>e</sup> form of y<sup>e</sup> Equation

$$x^m = ax^{m-1} + bx^{m-2} + cx^{m-3} + \&c,$$

and any Term of y<sup>e</sup> Series whose index is  $a + b + c$  be divided by y<sup>e</sup> precedent. The further you continue y<sup>e</sup> Series y<sup>e</sup> better is y<sup>e</sup> Approximation.

I think myself very oblig'd t'y<sup>e</sup> for y<sup>e</sup> account you gave me of M<sup>r</sup> Bradley's discovery, wick is indeed very noble, and pleased very much y<sup>e</sup> French Mathematicians, wick I communicat'd it to. It seems wondrous now that those who made some attempts to determine y<sup>e</sup> Parallax of y<sup>e</sup> fixt Stars. took no notice of y<sup>e</sup> successive propagation of y<sup>e</sup> Light. This is very surprising too what he observed of the different variation of declination, of y<sup>e</sup> Stars, greater for those wick are near y<sup>e</sup> Equinoxes, less for y<sup>e</sup> Stars near y<sup>e</sup> Solstices. It is plain, that the precession or change of Longitude being y<sup>e</sup> same for two Stars, the one in or near y<sup>e</sup> Solstitial Colure, the t'other in or near y<sup>e</sup> Equinoxial Colure, the mutation of Declination of this shall be greater than y<sup>e</sup> mutation of Declination of y<sup>e</sup> first. But, I suppose, M<sup>r</sup> Bradley took into consideration this Difference, wick arises only from their

situation and found the true mutation of Declination more different than it should be if no extraordinary cause did influe in it.

I render you thanks too for y<sup>e</sup> account of D<sup>r</sup> Halley's Letter to M<sup>r</sup> Maraldi. M<sup>r</sup> de Mairan is very satisfied and obliged to ye. He bid me to offer ye his Thanks and humble respects.

I long for receiving news of your book being under y<sup>e</sup> press. My thirst of seeing it is rather increased, than quenched, by the noble Theorem, you vouchsaf'd to communicate me. I found indeed a Demonstration of it, but as by chance, and, I think, not very general, and so your Method will give me a great pleasure.

Here is my demonstration.

It is known and easy to demonstrate that

$$\boxed{\dot{x}x^m \times 1 - x^q} \text{ is equal -}$$

$$\begin{aligned} & \overline{1 - x^{q+1}} \text{ in } - \frac{1}{m+q+1} x^m - \frac{m}{m+q+1 \cdot m+q} x^{m-1} \\ & - \frac{m \cdot \overline{m-1}}{m+q+1 \cdot m+q \cdot m+q-1} x^{m-2} \\ & - \frac{m \cdot \overline{m-1} \cdot \overline{m-2}}{m+q+1 \cdot m+q \cdot m+q-1 \cdot m+q-2} x^{m-3} \text{ \&c.} \end{aligned}$$

wich Series may be terminated to any Term, viz., to

$$\frac{m \cdot \overline{m-1} \text{ \&c usque ad } m-z+1}{m+q+1 \cdot m+q \text{ \&c usque ad } m+q-z+2} x^{m-z+1}$$

if you add this quantity

$$\frac{m \cdot \overline{m-1} \text{ \&c usque ad } m-z+1}{m+q+1 \cdot m+q \text{ \&c usque ad } m+q-z+2} \boxed{\dot{x}x^{m-z} \times 1 - x^q}$$

In the case of  $1-x=0$  all the terms become equal to nought, but this last quantity, and it is

$$\begin{aligned} & \boxed{\dot{x}x^m \times 1 - x^q} \\ & = \frac{m \cdot \overline{m-1} \text{ \&c usque ad } m-z+1}{m+q+1 \cdot m+q \dots m+q-z+2} \boxed{\dot{x}x^{m-z} \times 1 - x^q} \end{aligned}$$

Let  $m$  be equal  $z+r-1$ , and  $m+q+1$  be  $z+p-1$ , or  $q = p-r-1$ , you'll have

$$\left[ \dot{x} x^{z+r-1} \times 1 - x^{p-r-1} \right] \\ = \frac{z+r-1 \cdot z+r-2 \dots z+r-z}{z+p-1 \cdot z+p-2 \dots z+p-z} \left[ \dot{x} x^{r-1} \times (1-x)^{p-r-1} \right]$$

Then

$$\left[ \dot{x} x^{z+r-1} \times 1 - x^{p-r-1} \right] : \left[ \dot{x} x^{r-1} \times (1-x)^{p-r-1} \right] \\ :: \frac{z+r-1 \cdot z+r-2 \dots z+r-z}{z+p-1 \cdot z+p-2 \dots z+p-z} : 1 :: \frac{r \cdot r+1 \dots r+z-1}{p \cdot p+1 \dots p+z-1} A : A$$

I am with a great esteem and affection

Sir

Your most humble, most

Obedient Servant

G. CRAMER

(6)

*Cramer to Stirling, 1729*

Mr James Stirling at the Academy in  
little Tower Street  
London

Sir

I received indeed in due time your last letter, with the inclosed for Mr Nichol. Bernoulli which I sent him immediately; but several indispensable affairs, together with receiving no news from him, were the cause of my long delay in answering your most agreeable Letter. I began to reproach myself my Laziness, when your worthy friend came with your dear Letter to awake me. I'll be very glad to find some opportunity to show him, by any Service I am able to do him, how much I am sensible of your kindnesses to me.

I told you already I had no news from Mr Nich. Bernoulli, since I sent him your learned Letter. I believe he is meditating you an answer: however I write to him to warn him it is high time to do it. I received in the meanwhile several letters from his Uncle: Dr John Bernoulli, who is always

contriving again and again new Arguments for his Opinion about vivid forces. I don't know you have read what Mr 'S Gravesande publish'd in the *Journal Litteraire* about that matter. 'Tis all metaphysical reasoning, in answer chiefly to the late Dr Clarke and Mr MacLaurin.

I read with a great pleasure your Elegant Series for finding the Middle Uncia of any Power of a Binomial, and for summing a slow converging Series, but cannot imagine what principles have brought ye to these Series. 'Tis nothing like your Theorem for interpoling any Term in that Series

$$A, \frac{r}{p} A, \frac{r+1}{p+1} B, \frac{r+2}{p+2} C, \text{ \&c. :}$$

I sent all that to Mr Bernoulli.

I render ye thanks for the account you gave Mr Bernoulli of Mr Machin's Theorems. They seem indeed very well contrived for clearing Sr Isaac Newton's Theory of the Motion of the Moon and easily computing that Motion. I was mightily pleased with that Elegant improvement of Kepler's Proposition, of Areas described in Proportional Times, and the more pleased I was, that the Demonstration is so easy that I wondered no body, before Mr Machin, had thought of that Theorem.

I wrote you in so few words o<sup>r</sup> Mr Dan. Bernoulli's Way of approximating to y<sup>e</sup> greatest and smallest root of any given Equation by the help of a recurrent Series, that I was almost unintelligible. Now here are his own words. 'Methodus inveniendae minimae radiceis aequationis cujuscumque tam numericae tam algebraicae. Concilietur aequationi propositae haec forma  $1 = ax + bx^2 + cx^3 + ex^4 + \text{\&c.}$  Dein formetur Series incipiendo a tot terminis arbitrariis quot dimensiones habet Equatio, hac lege. ut si  $A, B, C, D, E$  denotent terminos se invicem directo ordine consequentes, sit ubique  $\Sigma = aD + bC + cB + eA + \text{\&c}$  sintque in hae Serie satis continuata duo termini proximi  $M$  &  $N$ , erit terminus antecedens  $M$  divisus per consequentem  $N$  proxime aequalis Radici minimae quaesitae.' And after some cautions to be observed in several cases he goes on. 'Ut inveniatur Radix aequationis maxima, Proposita sit aequatio Catholica sic disposita  $x^m = ax^{m-1} + bx^{m-2} + cx^{m-3} + \text{\&c}$  Formetur Series

incipiendo a tot terminis arbitrariis quot dimensionum est aequatio, eaque talis, ut si  $A, B, C, D, E$  denotent terminos directo ordine e Serie excerptos & contiguos, sit ubique  $\Sigma = aD + bC + cB + eA + \&c$ , sintque in hac Serie satis continuata duo termini proximi  $M$  &  $N$ , erit terminus  $N$  divisus per praecedentem  $M$  proxime aequalis radiei maximae.

The demonstration of wick I conceive to be thus. Let the Roots of the Equation  $1 = ax + bx^2 + cx^3 + \&c$  be  $\frac{1}{x}, \frac{1}{y}, \frac{1}{z}, \&c$  and of the Equation  $x^m = ax^{m-1} + bx^{m-2} + cx^{m-3} + \&c$  be  $x, y, z, \&c$ : and if the term  $M$  is in order  $l$  of the recurrent Series whose index is  $a + b + c + \&c$  this term  $M$  will be, for the values  $a, b, c, \&c$  of the first Equation  $\frac{p}{x^l} + \frac{q}{y^l} + \frac{r}{z^l} + \&c$ . and, for the values  $a, b, c, \&c$  in the second Equation  $px^l + qy^l + rz^l$ ; and the next term in order  $l + 1$ , and called  $N$  shall be, for the first Equation  $\frac{p}{x^{l+1}} + \frac{q}{y^{l+1}} + \frac{r}{z^{l+1}} + \&c$  and for the second Equation  $px^{l+1} + qy^{l+1} + rz^{l+1} + \&c$ . Now if  $x$  be the greatest and  $\frac{1}{x}$  the smallest root the greater is  $l$ , or the further is that term  $M$  from the beginning of the Series, the greater is  $\frac{p}{x^l}$  in comparison with the other terms  $\frac{q}{y^l} + \frac{r}{z^l} \&c$ , and  $\frac{p}{x^{l+1}}$  in comparison with  $\frac{q}{y^{l+1}} + \frac{r}{z^{l+1}} + \&c$ . So that if  $l$  be infinite the terms  $\frac{q}{y^l} + \frac{r}{z^l} \&c$  and  $\frac{q}{y^{l+1}} + \frac{r}{z^{l+1}} + \&c$  are not to be considered but  $\frac{p}{x^l}$  and  $\frac{p}{x^{l+1}}$  make up the Terms  $M$  and  $N$ , the former of wick being divided by the latter gives you  $x$ . In the other Equation  $px^l$  and  $px^{l+1}$  being infinitely greater than  $qy^l + rz^l + \&c$  and  $qy^{l+1} + rz^{l+1} + \&c$  make up the Terms  $M$  and  $N$ , and  $\frac{N}{M} = \frac{px^{l+1} + \&c}{px^l + \&c} = x$  the greatest root.

I am with a great respect

Sir

Your most humble and most

Obedient Servant

Geneva y<sup>e</sup> 26 Decemb 1729 N.S.

G. CRAMER.

As soon as yours and Mr de Moivre's books are printed, you'll oblige me very much to give notice of it to Mr Caille, that he may get them and send them to me. I believe he has changed his lodgings, but he uses to go to Bridge's Coffee house over against y<sup>e</sup> Royal Exchange.

(7)

*Cramer to Stirling, 1730*

Mr James Stirling F.R.S. at the  
Academy in little Tower Street  
London

Sir

As there is no less than a year, since I have no Letter from ye, I don't know, whether I must not fear the Loss of a Letter w<sup>ch</sup> I sent ye about that time, containing a Letter from Mr Nich. Bernoulli in answer to yours, together with a Copy of his Method for finding y<sup>e</sup> component quantities of a Binomium like this  $1 \pm z^n$  by the Division of the Circle.

Extraordinary businesses have, from that time hindred me always, from having the Pleasure of writing ye, and inquiring after the Philosophical and Mathematical news of w<sup>ch</sup> there is abundance in England in any time. I don't know whether your learned book about Serieses is published, but I wish and I hope it is, and y<sup>e</sup> Publick is not prived of your fine Inventions. I heard Mr de Moivre's book is out, but I have not seen it yet.

You know without any doubt, that Mr 'S Gravesande had made some little improvement to your method, given in your book *Enumeratio linearum 3<sup>ii</sup> Ordinis &c* for finding the difference of exponents Arithmetically proportional in an infinite Series formed from a given equation: w<sup>ch</sup> improvement he published at the end of his *Matheseos universalis Elementa*: but I found his Method wants yet a little correction, for it can induce into Error, if the given equation, besides  $x$  and  $y$  contains their fluxions. Let, for instance, the Equation be

$$\frac{-y^9}{a^5} + x^3 y \dot{y}^2 - 2x^3 y \dot{y} + x^4 \dot{y} + \frac{x^{14}}{b^{10}} = 0$$

and by Sr Isaac's Method of Parallelogram, you'll find in the Series resulting ( $y = Ax^n + Bx^{n+r} + \&c$ )  $n = 1$ , and substituting  $x$  instead of  $y$ , and  $\dot{x}$  instead of  $\dot{y}$ : the indices shall be 9.4.4.4.14. Whence, by Dr Taylor's Method,  $r$  being the common divisor is 1. By your method, the first term shall be  $AAx - 2Ax + 1x = 0$  or  $AA - 2A + 1 = 0$ , where  $A$  has two equal valors, and therefore, by your method  $r = \frac{m}{p} = \frac{1}{2}$ . Mr 'S Gravesande's Method gives for  $r$ 's value  $2\frac{1}{2}$ . But really  $r$  may be taken  $= 5$ , and the form of the Series is  $y = Ax + Bx^6 + Cx^{11} + \&c$ . This valor of  $r = 5$ , is deduced from this Rule, wick may be substituted to others. having found, by the Parallelogram, the greatest terms of the Equation, and thereby the valor of  $n$ ; see whether these terms give for  $y$ , or  $\dot{y}$ , or  $\ddot{y}$  &c many equal valors, and let  $p$  design the number of these equal valors of  $y$ , or  $\dot{y}$  &c. Then substitute for  $y$  and  $\dot{y}$ ,  $\ddot{y}$  &c,  $x^n$ ,  $x^{n-1}$ ,  $x^{n-2}$  &c and write down the indexes of all the terms. Subtract them all from  $y^e$  greatest, or subtract the smallest from all the others; according as the Parallelogram gave you the greatest or the least index. Divide the least of these differences by  $p$ , & of this so divided, and of all others, find the greatest common divisor. This shall be the valor of  $r$ .

So in the Example cited, the Parallelogram gives for the greatest terms of  $y^e$  Equation  $x^3y dy^2 - 2x^3y dy + x^4dy = 0$ , wick divided by  $x^3dy$ , gives  $ydy - 2y + x = 0$ , where  $y$  has not many equal values, Theref.  $p = 1$ . The indexes are 9.4.4.4.14. The difference 5.10. The common Divisor 5. Whence  $r = 5$ .

I wou'd gladly know from ye, how one can find the number of Roots of an exponential Equation, like this  $y^x = 1 + x$  for the method you give in the 6 Coroll. of  $y^e$  2<sup>nd</sup> Prop. of your book Enumeratio &c p. 18 does not succeed in this case.

It is a thing pretty curious, that in the Curve represented by that Equation  $y^x = 1 + x$ , or  $y = \frac{1}{1+x}$ , the abscissa being  $= 0$ , the ordinate  $y$  is not 1, but of a very different value, tho' it seems at the first sight, it must be 1, being  $1 + 0^{\frac{1}{0}}$ .

I have happily conserved a Copy of Mr Bernoulli's Letter,

so that I can send it ye, if you have not received y<sup>e</sup> original, w<sup>ch</sup> I pray, I may know from ye, as soon as you can without any trouble at all.

I am, with a great esteem and respect

Sir

Your most humble

Geneva, the 22 X<sup>bre</sup> 1730 N.S.      most obedient Servant

G. CRAMER.

(8)

*Stirling to Cramer, 1730*

Copy of a Letter sent to M<sup>r</sup> Cramer at  
Geneva    September 1730

Sir

I beg a thousand pardons for delaying so long to return you an answer. I was designing it every day but unluckily hindred by unexpected accidents. So that now I am quite ashamed to begin, and must intirely depend on your goodness.

I send two Copies of my Book, one for yourself and y<sup>e</sup> other for M<sup>r</sup> Bernoulli which I hope you will transmit to him along with the letter directed to him. I have left it open for your perusal, and you will find a letter which M<sup>r</sup> Machin sent me being an answer to what M<sup>r</sup> Bernoulli write about his Small Book.

The first part of my Book you see is about y<sup>e</sup> Suming of Series where I have made it my chief business to change them that converge slow into others that converge fast; but that I might not seem quite to neglect the suming of those which are exactly sumable, I have shown how to find a fluxionary Equation which shall have any proposed Series for its root, by the Construction of which Equation the series will be sumed in the simplest manner possible, I mean either exactly or reduced to a Quadrature perhaps, by which means I take this matter to be carryed farther than it was before: this you will see is the 15 Proposition and its Scholien I have taken an opportunity of clearing up a difficulty about the extracting the Root of a fluxionary Equation, which is the only one that Sir Isaac left to be done. This first part

has been written 8 or 9 years ago, so that if I were to write it again I should Scarce change anything in it; But indeed that is more than I can say for the Second part, because there was not above one half of it finished when the begining of it was sent to the Printer. And altho' I am not conscious of any Errors in it but Typographical ones, yet I am sensible that it might have been better done.

The 20 Prop: about y<sup>e</sup> Suming of Logarithms has been Considered by Mr De Moivre since y<sup>e</sup> publication of my Book, and he has found a Series more simple than mine which is as follows. Let there be as many naturall numbers as you please 1, 2, 3, 4 ...  $z$  whereof the last is  $z$ . Make  $l, z =$  Tabular log. of  $z$ ,  $l, c = \log.$  of 6.28318 which is the Circumference of a Circle whose Radius is unity,  $a = .43429 \dots$  which is y<sup>e</sup> reciprocal of y<sup>e</sup> Hyperbolick Log of 10. and y<sup>e</sup> sum of y<sup>e</sup> Logarithms of the proposed numbers will be <sup>1</sup>

:        :        :        :

whereas you will see that in my Series y<sup>e</sup> Numerators are y<sup>e</sup> alternate powers of 2, diminished by unity: the degree of convergency is y<sup>e</sup> same in both, and indeed there is seldome occasion for above three Terms, reckoning  $-za$  the first: Mr De Moivre is to publish this with his manner of finding it out, which is quite different from mine, which is done by an old and well known principle, namely the taking of the difference of the successive values of quantitys as you will see in y<sup>e</sup> Book, about which I shall be glad to have your opinion: and I hope you will write to me soon after this comes to hand, else I shall take it for granted that you have not forgiven me. I shall be always glad to hear of your wellfare, and to know your news of any kind whatsoever. I am with the greatest respect

D. Sir

Your most Obedient &

most humble Servant

London September 1730

JAMES STIRLING.

<sup>1</sup> The gap occurs in Stirling's copy of the letter.

(9)

*Cramer to Stirling, 1731*

Mr James Stirling R.S.S. at the Academy  
in little Tower Street  
London

Sir

I guess by the date of your Letter you must be very angry with me, thinking, as you may well, my negligence in returning you an Answer quite unpardonable. But I beseech you to believe, I cou'd not be so ungratefull as not to rendring you due thank for your fine present, wich I received but from five days. The chief reason of that accident is the forgetfullness of a Merchant to whom Mr Caille gave the two Exemplarys of your Book for sending them to me, then his sickness, then the violence of the winter, than I know not what, so that, to my great misfortune, they came here but the 12<sup>th</sup> of June. As soon as I received them, I sent Mr Bernoulli his Exemplary together with the Letter for him and the inclosed Letter of Mr Machin. And I resolved to write you even before the perusing of your book that I could justify myself of a so long and unexcusable delay.

As far as I can see, by a superficial Lecture of the Titles of your Propositions, this Treatise is exceedingly curious, and carries far beyond what has been done heretofore a Doctrine of the utmost importance in the Analysis. I rejoice beforehand, for the advantages I shall reap from an attentive Lecture of it, and I flatter myself you shall be so kind as to permit me to improve this benefit by the correspondence you vouchsafe to keep with me.

You shall know Mr N. Bernoulli has been this month elected Professor of the Civil Law, in his own University, wich I fear will perhaps interrupt his Mathematical Studies. I have perused, as you permitted, your Letter to him, and, in my opinion you are in the right as to your objections against his manner of interpoling the Series  $\frac{r \cdot r + b \cdot r + 2b \dots r + zb - b}{p \cdot p + b \cdot p + 2b \dots p + zb - b}$  by putting it equal to  $\frac{r \cdot r + b \dots p - b}{r + zb \dots zb + p - b}$

or  $= \frac{p+z b \dots z b+r-b}{p.p+b \dots r-b}$ , which cannot succeed but in some few cases, wick have no difficulties. His Theorem sent to Mr Montmort seems to be usefull in many cases. I have found a demonstration of it very simple, and made it more general, in that manner. The Series

$$\begin{aligned} & \frac{1}{a.a+b.a+2b \dots a+p-1b} \\ & - \frac{n}{a+c.a+c+b.a+c+2b \dots a+c+p-1b} \\ & + \frac{\frac{n}{1} \times \frac{n-1}{2}}{a+2c.a+2c+b \dots a+2c+p-1b} \\ & - \frac{\frac{n}{1} \times \frac{n-1}{2} \times \frac{n-2}{3}}{a+3c.a+3c+b \dots a+3c+p-1b} \\ & + \frac{\frac{n}{1} \times \frac{n-1}{2} \times \frac{n-2}{3} \times \frac{n-3}{4}}{a+4c.a+4c+b \dots a+4c+p-1b} - \&c. \end{aligned}$$

(by putting  $A=$ ,  $B=\frac{c-b}{2}A$ ,  $C=\frac{c-2b}{3}B$ ,  $D=\frac{c-3b}{4}C$ , &c and

$\overline{Az+Bz^2+Cz^3+Dz^4+\&c}^n = Hz^n + Iz^{n+1} + Kz^{n+2} + Lz^{n+3} + \&c$ ) will be reduced into this

$$\begin{aligned} & \frac{p.p+1.p+2 \dots p+n-1}{a.a+b \dots a+p+n-1b} H - \frac{p.p+1.p+2 \dots p+n}{a.a+b \dots a+p+nb} I \\ & + \frac{p.p+1 \dots p+n+1}{a.a+b \dots a+p+n+1b} K - \frac{p.p+1 \dots p+n+2}{a.a+b \dots a+p+n+2b} L \&c \end{aligned}$$

or, (if you like rather to have but the sign + and not alternately + & -) into This

$$\begin{aligned} & \frac{p.p+1.p+2 \dots p+n-1}{a+nc+p-1b.a+nc+p-2b \dots a+nc-nb} H \\ & + \frac{p.p+1.p+2 \dots p+n}{a+nc+p-1b \dots a+nc-n+1b} I \\ & + \frac{p.p+1.p+2 \dots p+n+1}{a+nc+p-1b \dots a+nc-n+2b} K + \&c \end{aligned}$$

where if  $c = b$ ,  $A$  being  $= b^n$ , and  $B = C = D = \&c = 0$  all the Series is reduced to the first term

$$\frac{p \cdot p+1 \cdot p+2 \dots p+n-1}{a \cdot a+b \cdot a+2b \dots a+p+n-1b} b^n,$$

and, moreover, if you put again  $p = 1$ , you'll have Mr Bernoulli's Theorem. I have also read over Mr Machin's Letter, but I cannot judge of their difference having not seen his Book. Mr Caille cou'd not find it. I am glad for what you say to Mr Bernoulli, he is preparing for the press a compleate Treatise about it. I conjure you to make me know as soon as it shall come forth, where it is printed, for I shall read it with a great pleasure.

I had willingly delayed this letter till I had some news for ye, but I chuse rather to send this empty answer, than to put off any longer to tell ye I am with the greatest esteem and respect

Sir

Your most humble, most obedient

Geneva 18<sup>th</sup> June 1731. and most faithfull Servant

G. CRAMER.

(10)

*Cramer to Stirling, 1732*

Mr James Stirling. R.S.S.  
at the Academy in little Tower Street  
London.

Geneve, ce 22<sup>e</sup> Fevrier, 1732.

Ne Soyés pas surpris, mon cher Monsieur, de recevoir si tard la Réponse à Votre chere Lettre du Mois de May 1731, puisqu'il n'y a que très peu de jours que Monsieur Bernoulli me l'a fait remettre. J'espère aussi que vous me permeterez de vous écrire dans ma Langue maternelle, puisque je sais que vous l'entendés fort bien. Et je crois vous ennuier moins en vous parlant une Langue qui vous est un peu étrangere qu'en vous obligeant à lire un Anglois aussi barbare que celui que je pourrois vous écrire. Je continue à vous rendre mille graces pour le present que vous avés daigné me faire de vôtre

excellent Ouvrage, dont je vous ai accusé la reception dans une Lettre que vous devés avoir reçu depuis l'envoy de la Vôtre. On ne peut rien trouver dans le livre que d'exquis pour ceux qui se plaisent aux Spéculations dont vous avés enrichi les Mathematiques. Je n'en dirai davantage de peur de paroître vous flatter, quoiqu'assurement ce que j'en pourrois dire seroit fort au dessous de ce que j'en pense, et de ce que j'en devrois dire.

La Regle de Dr Taylor pour trouver la forme d'une Serie doit être proposée, comme vous le remarqués sous une forme différente de celle qu'il a donnée, en ce que  $r$  doit être, non le plus grand commun diviseur des indices, mais bien celui des Differences des Indices. Mais pour qu'elle puisse s'étendre à tous les cas possibles, Mr Gravesande dit qu'ayant substitué dans l'Equation,  $Ax^n$  au lieu de  $y$  &c il faut chercher la Valeur de  $A$  & s'il se trouve qu'il ait plusieurs valeurs égales, il faut prendre pour  $r$  le plus grand commun diviseur des Differences, mais tel qu'il mesure la plus petite par le nombre des valeurs égales de  $A$  ou par un multiple de ce nombre Il en donne l'exemple suivant.

$$\frac{x^{14}}{b^{10}} + x^3y - 2x^2y^2 + xy^3 - \frac{y^9}{a^5} = 0$$

que la substitution de  $Ax^n$  au lieu de  $y$ , change en

$$\frac{x^{14}}{b^{10}} + Ax^{n+3} - 2A^2x^{2n+2} + A^3x^{3n+1} - \frac{A^9x^{9n}}{a^5} = 0$$

Donc les indices sont 14,  $n+3$ ,  $2n+2$ ,  $3n+1$ ,  $9n$ . Par le Parallelogramme de Mr Newton on trouve pour la forme de la suite d'autant plus convergente que  $x$  est moindre,  $n=1$ , ce qui change les indices en 14, 4, 4, 4, 9. Otant le plus petit des autres, les differences sont 5, 10. Le plus grand commun diviseur est 5; Ainsi selon la Regle de Mr Taylor corrigée, la forme de la suite doit être  $Ax + Bx^6 + Cx^{11} + \&c$ . Mais selon Mr S Gravesande si l'on veut determiner la valeur de  $A$  par le moyen des plus grands termes de l'equation qui sont  $Ax^{n+3} - 2A^2x^{2n+2} + A^3x^{3n+1}$ , ou  $Ax^4 - 2A^2x^4 + A^3x^4$  égalés a zero et divisés par  $x^4$  on trouve qu'il a 2 valeurs egales.

Donc  $r$  doit diviser les 2 differences 5 & 10, et entr'autres la plus petite par 2 ou 4, ou 6, &c.

Ainsi  $r$  doit être  $2\frac{1}{2}$ , et la forme de la Serie sera

$$Ax + Bx^{\frac{3}{2}} + Cx^6 + Dx^{\frac{8}{2}} + \&c.$$

Mais cette Regle de Mr 'S. Gravesande ne paroît pas encore assés generale, car il peut aisement arriver dans les Equations

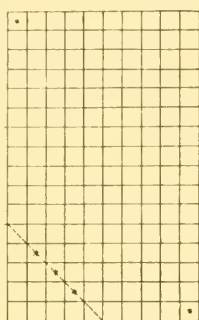


FIG. 21.

fluxionelles que  $A$  ait plusieurs Valeurs égales, sans qu'il y faille faire aucune attention. Ainsi quoique sa Regle donne toujours une Suite propre à déterminer la Valeur de  $y$ , cependant elle ne donne pas toujours la plus simple. Il falloit donc établir la Regle ainsi. Si les plus grands termes de l'Equation déterminés par le Parallelogramme de Mr Newton, etant égalés a zero, font une Equation dans laquelle  $y$  ou quelqueune de ses Fluxions ait plusieurs Valeurs égales,

Divisés la plus petite difference des Indices par le nombre de ces Valeurs égales, Et le plus grand commun diviseur du Quotient et des autres Differences sera le nombre  $r$  cherché. Par exemple, si l'Equation cy-dessus avoit été

$$\frac{\dot{x}x^{14}}{b^{10}} + \dot{x}^4\dot{y} - 2x^3\dot{y}y + x^3\dot{y}^2y - \frac{\dot{x}y^9}{a^5} = 0$$

on auroit trouvé la même valeur de  $n=1$ , les mêmes indices 14, 4, 4, 4, 9, les mêmes differences 5, 10, que cy-devant, &  $A$  auroit aussi deux Valeurs. Donc selon la Regle de Mr 'S Gravesande on auroit la même forme de Serie,  $Ax + Bx^{\frac{3}{2}} + Cx^6 + \&c$ , Au lieu que suivant la Regle que je viens de poser, les plus grands termes de l'Equation  $x^4y - 2x^3\dot{y}y + x^3\dot{y}^2y$ , égalés a zero et divisés par  $x\dot{y}$  donnent  $x - 2y + \dot{y}y = 0$  qui ne donne pas deux valeurs egales de  $y$  ou  $\dot{y}$ . Ainsi il faudra simplement prendre pour  $r$  le plus grand commun diviseur 5 des differences 5, 10, Et la forme de la Serie est  $Ax + Bx^6 + Cx^{11} + \&c$ . Ainsi si l'on calcule selon la forme de Mr 'S Gravesande, on trouve tous les Coefficiens des Termes pairs égaux a zero.

C'est là la Regle Generale. Mais il se rencontre quelquefois des cas, où il n'est pas si facile de l'appliquer. Les Termes placés sur le Parallelogramme de Mr Newton peuvent se trouver sur une même ligne Verticale. Alors on ne peut en les

comparant déterminer la Valeur de l'exposant  $n$ . Mais en supposant que le terme le plus grand est celui qui a le plus grand ou le plus petit exposant selon qu'on veut que la Suite converge, d'autant plus que  $x$  est plus petite ou plus grande: On détermine par cette supposition la Valeur de  $n$  & la forme de l'Equation. Mais la valeur du premier  $r$  et souvent de quelques autres coefficients reste indéterminée.

Done si tous les termes placés sur le Parallelogramme de Mr Newton se trouvent dans une même ligne oblique, ou ce qui revient au même, lorsqu'ayant substitué dans l'Equation  $Ax^n$  au lieu de  $y$ , &  $nAx^{n-1}$  au lieu de  $\dot{y}$ , & les indices des termes resultans se peuvent tous rencontrer entre les Termes d'une Progression Arithmetique: alors l'equation est à une ou plusieurs Paraboles, ou bien à une ou plusieurs hyperboles, qu'il est facile de déterminer.

Soit par exemple l'equation  $2x\dot{x} - 4\dot{x}\sqrt{ay} - 15a\dot{y} = 0$  & après la substitution de  $Ax^n$  au lieu de  $y$ , les indices seront  $1, \frac{1}{2}n, n-1$ , qui sont en Progression Arithmetique, les supposant égaux on trouve  $n = 2$ . Soit donc  $y = Ax^2$  et après la Substitution l'equation devient  $2\dot{x}x - 4\dot{x}x\sqrt{aA} - 30aA\dot{x}x = 0$  ou, divisant par  $\dot{x}x$ ,  $2 - 4\sqrt{aA} - 30aA = 0$ . Donc les Racines sont  $1 - 5\sqrt{aA} = 0$ , &  $1 + 3\sqrt{aA} = 0$ . Dans ces Racines mettant au lieu de  $A$  sa valeur  $\frac{y}{x^2}$ , elles se changent en  $1 - 5\sqrt{\frac{ay}{x^2}} = 0$  &  $1 + 3\sqrt{\frac{ay}{x^2}} = 0$  dont la multiplication produit

$$xx - 2x\sqrt{ay} - 15ay = 0$$

qui est la fluente de la fluxion proposée

$$2\dot{x}x - 4\dot{x}\sqrt{ay} - 16a\dot{y} = 0$$

Or cette equation designe deux demi Paraboles decrites sur le même axe & du même Sommet, les branches tirant d'un même côté, dont la superieure a pour Parametre  $25a$ , & l'inferieure  $9a$ ; L'abscisse commune est  $y$ , & l'ordonnée de la premiere est  $x$ , celle de la seconde  $-x$ .

Quant à l'Equation de la Courbe  $y^x = \overline{1+x}$ , voici la difficulté qui m'avoit porté à vous demander si elle n'a qu'une ou deux branches. C'est que quand  $x$  est un nombre pair, il semble que  $y$  doive avoir 2 Valeurs égales, l'une positive l'autre négative, puisque toute puissance paire a deux Racines. Par

Exemple quand  $x=2$ , l'equation devient  $y^2=3$ , Donc  $y = + \sqrt{3}$  &  $- \sqrt{3}$ . Mais quand  $x$  est impair, je ne trouve plus qu'une Valeur pour  $y$ . Car, par exemple, quand  $x=3$ , l'equation  $y^3=4$  n'a qu'une racine réelle, scavoir  $y = \sqrt[3]{4}$  les deux autres Racines  $y = -\frac{1}{2}\sqrt[3]{16} + \sqrt{-\frac{3}{4}\sqrt[3]{16}}$ , &  $y = -\frac{1}{2}\sqrt[3]{16} - \sqrt{-\frac{3}{4}\sqrt[3]{16}}$  étant imaginaires. Il semble donc qu'outre le Rameau ou la Branche qui est du Côté où l'on prend les  $y$  positives, l'Equation designe quelques points par-ci par-là du côté negatif, & non pas une branche entière et continue ce qui est absurde. La difficulté est la même quand  $1+x$  est negatif. Car a en juger par l'Equation il semble que  $y$  aura alternativement des Valeurs réelles et imaginaires, selon que  $x$  sera impair ou pair. La même difficulté se presente dans toutes les Courbes exponentielles sans en excepter la Logarithmique. Je ne vois pas que personne ait donné là dessus quelque éclaircissement. Je souhaiterois que vous vous donnassiez la peine de m'expliquer un peu plus au long sur quel fondement il vous paroît que  $y$  a deux valeurs égales mais avec des Signes contraires.

En reduisant en suite l'equation  $y^x = 1 + x$  je crois qu'on ne trouve qu'une seule suite, ce qui n'indiqueroit qu'une valeur. Mais le Calcul est si long, que je n'ai ni le courage ni le tems de l'entreprendre pour mieux m'assurer de ce soupçon.

Votre détermination de la Valeur de  $y$  quand  $x$  est zero, est conforme à celle que j'ai aussi trouvée par la même manière et encore par quelques autres. Par Exemple. On peut ainsi construire la Courbe

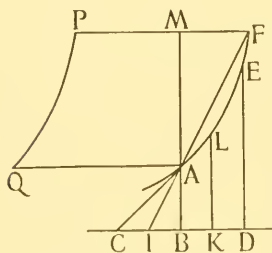


FIG. 22.

Sur l'Asymptote  $CD$  soit decrite la Logarithmique, dont la sontangente soit l'unité. Soit l'Ordonnée  $AB$  égale à la Soutangente ou à l'unité. Soit prise une abscisse quelconque  $AM = x$ . Pour trouver l'Ordonnée correspondante  $MP = y$ , je trace la perpendiculaire  $PMF$  rencontrant la Logarithmique au point  $F$ . Par les points  $F$  &  $A$  je tire la Chorde ou

secante  $FAI$ , qui rencontre l'Asymptote en  $I$ . Je prends  $BK = BI$  et élevant la perpendiculaire  $KL$  je fais  $MP = KL$ . Le point  $P$  est à la Courbe  $PQ$  cherchée. Car puisque

$AM = x$ ,  $BM = x + 1$ , &  $MF = \sqrt{Lx + 1}$ . Soit  $MP = LK = y$  &  $BK = BI = Ly$ . Les Triangles Semblables  $AMF$ ,  $ABI$  donnent  $FM (\sqrt{Lx + 1}) : MA (x) :: BI (Ly) : BA (1)$  Donc  $xLy = \sqrt{Lx + 1}$ , ou  $y^2 = x + 1$ . Or quand  $x = 0$  la Secante  $FAI$  devient la Tangente  $AC$ , & prenant  $BD = BC = 1$  (la soutangente) la Perpendiculaire  $DE$  (qui est le nombre dont le Logarithme est l'unité = 2.718281828459 &c) sera égale à l'Ordonnée  $AQ$ .

N.B. que cette Construction ne donne qu'une branche pour la Courbe sc.  $PQ$ .

Mais ce qui forme une nouvelle difficulté, c'est qu'en cherchant la Soutangente au point  $Q$  il semble qu'il y ait deux ou 3 rameaux qui se coupent en ce point là. Car l'expression

generale de la soutangente est  $\frac{xx \cdot \sqrt{1+x}}{x-1+x \cdot l \sqrt{1+x}}$  Or cette

expression devient (en substituant au lieu de  $x$  la valeur = 0)  $\frac{0}{0}$ .

Donc suivant l'art. 163 de l'Analyse des infiniment petits, prenant la Differentielle ou fluxion du Numerateur et du

Denominateur on trouve la soutangente au point  $Q = \frac{3xx + 2x}{-l(1+x)}$

qui est encore  $\frac{0}{0}$ . Donc differentiant de nouveau, on trouve

cette soutangente =  $-6xx - 8x - 2 = -2$  (puisque  $x = 0$ ). Or

les Auteurs posent qu'on n'est obligé à ces differentiations que lorsque 2 ou plusieurs Rameaux de Courbe se coupent dans le point où l'on cherche la soutangente Voyés Memoires de l'Academie de Paris. Année 1716 p. 75 & Année 1723 pag. 321. Edit. de Coll. Voyés aussi Fontenelle Elements de la Geometrie de l'infini, p. 418 & 99.

Votre Probleme du jet des Bombes est de la derniere importance par raport à cette branche de la Mechanique. Je serai infiniment curieux d'apprendre le resultat de vos Experiences & de Vos Calculs. J'ai lu cet article de votre Lettre à plusieurs de mes Amis Officiers d'Artillerie, chès qui il a excité une merveilleuse curiosité. Ce que vous dites de la facilité de votre solution ne pique pas moins la mienne, puisque la Solution de M<sup>r</sup> Jean Bernoulli (Acta Erud. 1719. p. 222, & 1721. p. 228) est si compliquée et inaplicable à la pratique. Je vous supplie, si vous avés composé quelque chose là dessus de daigner me la communiquer.

Je voudrois bien en échange de votre belle Lettre vous indiquer aussi quelquechose digne de votre attention. Mais il n'est pas donné à tout le monde de Voler si haut. Je me rabaisse à de plus petits Sujets. Voici un Problème qui m'a occupé ces jours passés, et qui sera peut-être du gout de Mr de Moivre. Vous ne savés peut-être pas ce que nous appellons en Francois le jeu du Franc Carreau. Dans une chambre pavée de Carreaux, on jette en l'air un Ecu. S'il retombe sur un seul carreau, on dit qu'il tombe franc, et celui qui l'a jetté gagne. S'il tombe sur deux ou plusieurs Carreaux, c'est à dire, s'il tombe sur la Raye qui separe deux Carreaux, celui qui l'a jetté perd. C'est un Problème à resoudre & qui n'a point de difficulté. Trouver la Probabilité de gagner ou de perdre, Les Carreaux & l'Ecu étant données. Mais si au lieu de jeter en l'air un Ecu qui est rond, on jettoit une Pièce Quarrée, Le Problème m'a paru assés difficile, soit qu'il le soit naturellement, soit que la voye par laquelle je l'ai resolu ne soit pas la meilleure. Au reste j'ai reçu le Livre que Mr de Moivre m'a envoyé en présent. J'ai pris la Liberté de lui en faire mes remerciemens dans une Lettre dont j'ai chargé un jeune homme d'ici, qui est parti il y a quelques mois pour l'Angleterre. Je ne scais s'il la lui aura remise n'en ayant eu depuis aucune nouvelle. Je vous prie, quand vous le verrés de vouloir bien l'assurer de mes humbles respects, & de ma reconnoissance. Temoignés lui combien je suis sensible aux Marques publiques qu'il m'a données de son amitié. Il ne sera pas trompé dans sa Conjecture, quand il a cru que la 2<sup>e</sup> Methode de Mr Nicolas Bernoulli est la même que celle de Mr Stevens. Il y a plus d'un an que je n'ai aucune nouvelle de ce dernier. Sa nouvelle Profession l'occupe entièrement. Il a pourtant reçu votre Livre avec vos Lettres, et vous aura sans doute repondu. Je suis avec une estime et une consideration toute particuliere

Monsieur

Votre très humble, & très obéissant Serviteur

G. CRAMER.

(11)

*Cramer to Stirling, 1733*

Mr James Stirling. F.R.S.  
at the Academy in little Tower Street  
London

Monsieur

Voici une Lettre que je viens de recevoir pour vous de la part de Mr Nicol. Bernoulli. Elle est venue enfin après s'être fait longtems attendre. Un nombre considerable d'occupations m'empêche d'avoir l'honneur de vous écrire plus au long. Voici seulement un Extrait de ce qu'il me marque touchant sa nouvelle Manière de calculer les Numerateurs des fractions simples auxquelles se réduit la fraction  $\frac{1}{z^{2n} + 2tz^n + 1}$ . Soit supposé

$$\frac{1}{z^{2n} + 2tz^n + 1} = \frac{e - fz}{1 - xz + cz^2} +$$

$$\frac{\alpha + \beta z + \gamma z^2 + \delta z^3 + \dots + \mu z^{n-2} + Mz^{n-1} + \dots + Cz^{2n-5} + Bz^{2n-4} + Az^{2n-3}}{1 + az + bz^2 + cz^3 \dots + rz^{n-3} + sz^{n-2} + tz^{n-1} + sz^n \dots + az^{2n-3} + z^{2n-2}}$$

et reduisant ces deux fractions au commun denominateur, en multipliant en Croix, & faisant  $\alpha + e = 1$ , & les autres coefficients = 0 on aura les Equations de la Tabl. I lesquelles après avoir substitué pour  $x, ax, bx, cx$ , &c respectivement  $a, 1+b, a+c, b+d$  &c selon la nature de la suite récurrente,  $1, a, b, c, d$ , &c se changeront en celles de la Tabl. II

Tabl. I

$\alpha + e = 1$	$A - f = 0$
$\beta - \alpha x + ae - f = 0$	$B - Ax + e - af = 0$
$\gamma - \beta x + \alpha + be - af = 0$	$C - Bx + A + ae - bf = 0$
$\delta - \gamma x + \beta + ce - bf = 0$	$D - Cx + B + be - cf = 0$
$\epsilon - \delta x + \gamma + de - cf = 0$	$E - Dx + C + ce - df = 0$
&c	&c

Tabl. II

$\alpha = 1 - e$	$A = f$
$\beta = a - 2ae + f$	$B = 2af - e$
$\gamma = b - 3bc + 2af - e$	$C = 3bf - 2ae + f$
$\delta = c - 4ce + 3bf - 2ae + f$	$D = 4cf - 3be + 2af - e$
$\epsilon = d - 5de + 4cf - 3bc + 2af - e$	$E = 5df - 4ce + 3bf - 2ae + f$
$\cdot \quad \cdot \quad \cdot$	$\cdot \quad \cdot \quad \cdot$
$\mu = s - \overline{n-1}se + \overline{n-2}rf$	$M = \overline{n-1}sf - \overline{n-2}re$
$\quad \quad \quad - \overline{n-3}qe + \&c.$	$\quad \quad \quad + \overline{n-3}qf - \&c.$
$M = t - nte + \overline{n-1}sf$	$\mu = ntf - \overline{n-1}se$
$\quad \quad \quad - \overline{n-2}re + \&c.$	$\quad \quad \quad + \overline{n-2}rf - \&c.$

Ces deux différentes valeurs de  $M$  égalées ensemble donnent  $t - nte = 0$ , ou  $e = \frac{1}{n}$  & les deux valeurs de  $\mu$  donnent  $s = ntf$  ou  $f = \frac{s}{nt}$ , comme j'ai trouvé par induction dans la Solution de mon Probl. 5.

Je vous supplie, Monsieur, de vouloir bien me faire la grace de me donner de Vos nouvelles, & de m'informer de ce qui s'est publié nouvellement en Angleterre en fait de Philosophie & de Mathématique. Soyés persuadé que je suis avec une extrême consideration & un Veritable attachement,

Votre très humble & très obéissant

Serviteur

G. CRAMER.

Geneve ce 10<sup>e</sup> Avril, 1733

## IV

### N. BERNOULLI AND STIRLING

(1)

*N. Bernoulli to Stirling, 1719*

D<sup>no</sup> mihi plurimum colende

PERGRATUM mihi fuit nudius tertius accipere epistolam tuam, qua me ad mutuum epistolarum com̄ercium invitare voluisti, gaudeoque quod ea, de quibus ante hac Venetiis egimus, consideratione tua digna esse judices, quia igitur ea tibi in memoriam revocari cupis petitioni tuae libenter morem geram, quod attinet primo ad difficultatem illam, quam de resistentia pendulorum movebam, ea huc redit. Posita gravitatis vi uniformi et resistentia proportionali velocitati, non potest corpus grave oscillari in Cycloide; hoc quidem inveni per calculum, sed quomodo ista impossibilitas *a priori* ex rationibus physicis demonstrari possit, adhucdum ignoro. Rogo igitur ut hanc rem sedulo examines et quaeras constructionem Curvae, in qua abscissis denotantibus spatia oscillatione descripta (i.e. arcus Cycloidis interceptos inter punctum quietis et punctum quodvis ad quod mobile oscillando pertingit) applicatae denotent resistentiam vel velocitatem mobilis in fine illorum spatiorum. D<sup>ns</sup> Newtonus pag. 282. dicit hanc Curvam *proxime esse Ellipsi*. Problema quod à D<sup>no</sup> Taylor Geometris propositum mecum com̄unicavit D. Monmort, est sequens. Invenire per quadraturam circuli vel hyperbolae

fluentem hujus quantitatis  $\frac{\delta z^\lambda{}^{q-1}}{e + fz^q + gz^{2q}}$ , ubi  $\delta$  significat numerum quemlibet integrum affirmativum vel negativum, et  $\lambda$  numerum aliquem hujus progressionis 2, 4, 8, 16, 32 &c, petitur autem, ut hoc fiat sine ulla limitatione per radices

imaginarias. Denique quod attinet ad Theorema Patruī mei pro conjiciendis Curvarum arcibus in Series convergentes, tuamque contra ejus generalitatem factam oppositionem, in ea re adhucdum tecum dissentio, et in mea opinione firmatus sum, postquam nuper exemplum à te oblatum, et alia calculo subluxi; deprehendi enim seriem, licet in infinitum abeat, tamen esse summabilem, si area invenienda sit quadrabilis. De rebus aliis novis Mathematicis aut Philosophicis nihil comunicandum habeo, nisi quod Patruus meus miserit Lipsiam solutionem Problematis D<sup>i</sup> Taylori (quod et ego jamdudum solvi) cum subjuncta appendice infra scripta. Quod superest Vale et fave.

Dabam Patavii d. 29 Apr. 1719

Ill<sup>mus</sup> Polenus me enixe

D<sup>nis</sup> Tuæ

rogavit ut suis verbis tibi

Servo humillimo

plurimam Salutem dicerem

NICOLAO BERNOULLI

### Appendix Patruī

Adjicere lubet quaedam mihi inventa Theoremata, quæ in reductionibus utilitatem suam habent non exiguam. Demonstrationes eorum brevitatis gratia jam supprimo: Erunt inter Geometras qui facile invenient, quocirca illis eas relinquo.

*Definitio.* Per  $q$  et  $l$  intelligo numeros qualescunque integros, fractos, affirmativos, negativos, rationales, irracionales. Per  $\rho$  intelligo tantum numerum integrum et affirmativum, vel etiam cyphram. Sed per  $n$  et  $k$  intellectos volo numeros quoslibet integros affirmativos exclusa cyphra.

Theorema I  $\int ax : (e + fxe^l)^{\frac{1}{q}+k}$  est algebraice quadrabilis.

Theor. II Generalius,  $\int x^{\rho l} dx : (e + fxe^l)^{\frac{1}{q}+k+s}$  est algebraice quadrabilis.

Theor. III  $\int x^{kq-1} dx : (e + fxe^l)^{-\frac{1}{q}+k+s}$  est algebraice quadrabilis: Adeoque existente  $\rho = 0$ , erit etiam

$$\int x^{kq-1} dx : (e + fxe^l)^{-\frac{1}{q}+k}$$

algebraice quadrabilis.

Theor. IV  $\int x^{\rho q} dx : (e + f x^q)^n$  dependet à quadratura hujus  

$$\int dx : (e + f x^q).$$

Theor. V  $\int x^{-\rho q} dx : (e + f x^q)^n$  dependet à quadratura ejusdem  

$$\int dx : (e + f x^q).$$

Theor. VI  $\int x^{\rho q + l} dx ; (e + f x^q)^n$  dependet à quadratura hujus  

$$\int x^l dx : (e + f x^q).$$

Theor. VII Sumtis  $\delta$  et  $\lambda$  in Casu Taylori erit

$$\int z^{\frac{\delta}{\lambda} q - 1} dz : (e + f z^q)^n$$

quadrabilis per circulum vel hyperbolam.

Corolloria quae ex hisce Theorematibus deduci possent pulchra et miranda non minus quam utilia nunc omitto, sicut et plura alia ad quadraturarum reductionem spectantia, quae olim inveni ac passim cum Amicis comunicavi. Ex. gr. Ex collatione *Theorr.* V et VI sequitur inveniri posse duos coefficients  $\alpha$  et  $\beta$ , ita ut  $\int (\alpha x^{-\rho q} + \beta x^{\rho q + q}) dx : (e + f x^q)^n$  sit algebraice quadrabilis.

(2)

*Bernoulli to Stirling, 1729*

Viro Clarissimo Jacobo Stirling

S.P.D. Nic. Bernoulli.

Pergrata fuit epistola, quam per communem amicum D. Cramerum mihi haud pridem transmisisti et ad quam citius respondissem, si per varia impedimenta licuisset. Gaudeo te valere et rem Mathematicam per impressionem libri de summatione et interpolatione Serierum novis inventis locupletare. Gratias tibi ago pro illis quae prolixè narrasti de nova theoria Lunae à D. Machin inventa, cujus hac de re libellum nuperrime mihi donavit D. de Maupertuis, qui nunc apud nos versatur. Pauca quidem in eo intelligo, quia nullam adhuc operam

collocavi in lectione tertii libri Principiorum D. Newtoni; videris tamen mihi haud recte in epistola tua explicuisse quid ipse vocat *an Equant*. Verba sua sunt haec:

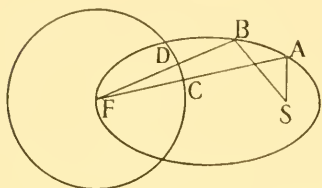


FIG. 23.

‘he constructs a figure whose Sector  $CDF$  is proportional to the angle  $ASB$ , and finds the point  $C$  which will make the figure  $CD$  nearest to a Circle’. Existimo dicendum fuisse ‘he constructs a figure, whose Sector  $CDF$  is equal to the area  $ASB$ ,

and finds the point  $F$  which will make the figure  $CD$  nearest to a Circle.' Ceterum etiam si inveniatur punctum aliquod  $F$  ex quo velocitas Planetæ in utraque apside constituti eadem appareat ex hoc non sequitur æquantem  $CD$  maxime accedere ad circulum, vel punctum  $F$  esse illud, ex quo motus Planetæ maxime uniformis appareat, ut D. Machin asserit pag. 41. Nam locus ex quo Planeta in  $A$  et  $P$  (fig. seq.) constitutus æque velox apparet non est unicuique punctum  $F$  sed integra linea tertii ordinis  $FAffPf$  cujus æquatio est

$$\overline{a-x}, \overline{yy} = \overline{a+b-x}, \overline{b-x}, \overline{x}$$

positis  $AS = a$ ,  $SP = b$ ,  $Ag = x$ ,  $gf = y$ . In hac igitur linea  
et quidem in ejus ramo  $Pf$  datur fortassis punctum  $f$ . ex quo

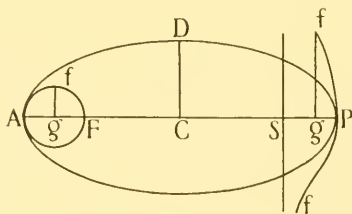


FIG. 24.

Planeta apparet aequæ velocis in tribus punctis  $A$ ,  $P$ , et  $D$ : adeoque ejus motus magis regularis vel uniformis quam ex puncto  $F$ . In ead. pag. 41. lin. 16 omiſſa eſt vox reciprocall; præter hunc errorem in eadem pag. notavi, quod Auctor videatur committere paralogiſmum, dum areas deſcriptas à corpore moto per arcum  $AR$  circa puncta  $S$  et  $F$ , item areas deſcriptas à lineis  $F\rho$  et  $FR$  dicit eſſe in duplicata ratione

perpendicularium in tangentem (ad punctum  $R$ ) demissarum ex  $S$  et  $F$ ; haec enim ratio obtinet tantum in harum arearum fluxionibus, à quarum proportionalitate ad proportionalitatem ipsarum arearum argumentari non licet, ut seis me olim quoque ex alia occasione monuisse; nihilominus consequentia, quod area à linea  $F\rho$  descripta aequalis sit areae à linea  $SR$  descriptae vera manet. Theorema illud, quod corpus ad duo fixa puncta attractum describat solida aequalia circa rectam conjungentem illa duo centra virium temporibus aequalibus, verum esse deprehendo. Reliqua examinare non vacat.

Theorema tuum pro interpolatione Series  $A, \frac{r}{\rho} A, \frac{r+1}{\rho+1} B, \frac{r+2}{\rho+2} C, \frac{r+3}{\rho+3} D$ , &c per quadraturas Curvarum deduci potest ex isto altero theoremate quod ante 19. annos cum D. de Monmort comunicavi,

$$\frac{1}{a} - \frac{n}{a+b} + \frac{n \cdot n-1}{1 \cdot 2 \cdot a+2b} - \frac{n \cdot n-1 \cdot n-2}{1 \cdot 2 \cdot 3 \cdot a+3b} \\ + \frac{n \cdot n-1 \cdot n-2 \cdot n-3}{1 \cdot 2 \cdot 3 \cdot 4 \cdot a+4b} - \&c = \frac{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \dots nb^n}{a \cdot a+b \cdot a+2b \dots a+nb}.$$

Sed et sine quadraturis interpolatur facillime Series  $A, \frac{r}{\rho} A, \frac{r+b}{\rho+b} B, \frac{r+2b}{\rho+2b} C, \frac{r+3b}{\rho+3b} D$ , &c ponendo

$$\frac{r \cdot r+b \cdot r+2b \dots r+zb-b}{\rho \cdot \rho+b \cdot \rho+2b \dots \rho+zb-b} = \frac{r \cdot r+b \cdot r+2b \dots \rho-b}{r+zb \cdot r+zb+b \dots zb+\rho-b}$$

$$\text{vel etiam} = \frac{\rho+zb \cdot \rho+zb+b \dots zb+r-b}{\rho \cdot \rho+b \cdot \rho+2b \dots r-b},$$

prout  $\rho$  major vel minor est quam  $r$ .

Ex. gr. Si  $z = 2\frac{1}{2}$  erit terminus inter tertium

$$\frac{r+b}{\rho+b} B \text{ et quartum } \frac{r+2b}{\rho+2b} C \text{ medius} =$$

$$\frac{r \cdot r+b \cdot r+2b \dots \rho-b}{r+2\frac{1}{2}b \cdot r+3\frac{1}{2}b \dots 1\frac{1}{2}b+\rho} \text{ vel } \frac{\rho+2\frac{1}{2}b \cdot \rho+3\frac{1}{2}b \dots 1\frac{1}{2}b+r}{\rho \cdot \rho+b \cdot \rho+2b \dots r-b}.$$

Aliud vero est interpolare ejusmodi Series quando valor ipsius  $z$  non est numerus integer, aliud invenire per approximationem aliquam earundem Serierum terminos non tantum

quando  $z$  est numerus fractus, sed et quando differentia inter  $p$  et  $r$  est numerus magnus, quod ultimum, ut et valorem Seriei alicujus lente convergentis, ope Serierum quarundam infinitarum promte convergentium à te inveniri, ex litteris D<sup>ni</sup> Cramer intellexi, quarum Serierum demonstrationem libenter videbo.

Optarem spei tue satisfacere tibi vicissim impertiendo nova quaedam inventa, sed dudum est quod Mathesis parum à me excolitur, nec nisi in gratiam amicorum me subinde ad solutionem quorundam Problematum accinxi, quorum solutiones in Schedis meis dispersae latent, et quoad maximam partem vix tanti sunt ut tecum communicari mereantur. D<sup>nm</sup> Cramer rogavi, ut tibi transmittere velit Specimen methodi meae (Pembertiana multo facillioris et ejus ipsum partieipem feci) resolvendi fractionem  $\frac{1}{1 + qz^n + z^{2n}}$  in fractiones hujus formae  $\frac{a + bz}{1 \pm cz + zz}$ .

Dñs de Maupertuis Patruo meo nuper proposuit sequens Problema:  $A$  et  $B$  sunt duo ignes quorum intensitates sunt ut  $p$  ad  $q$ , quaeritur per quam Curvam  $CD$  homo in dato loco  $C$

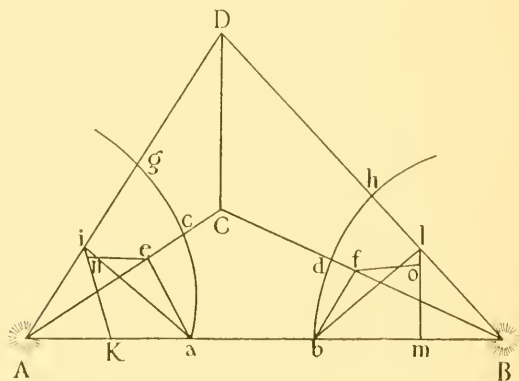


FIG. 25.

constitutus recedere debeat, ut sentiat minimum calorem, posito rationem cujusque ignis in objectum aliquod esse in ratione reciproca duplicata distantiarum.

Hujus Problematis sequentem constructionem inveni.

Centris  $A$  et  $B$  describantur circuli  $aeg$ ,  $bdlh$  aequalium



(3)

*Stirling to Bernoulli, 1730*

Copy of a Letter sent to M<sup>r</sup> Nicholas  
Bernoulli September 1730

Sir

I was very glad to hear of your welfare by your most obliging Letter and have delayed answering it hitherto for no other reason but that I might be able at length to answer you in every particular: for seeing you desired the Demonstrations of the two Series which M<sup>r</sup> Cramer sent you, and these Demonstrations are such as could not be conveniently brought within the bounds of a Letter, I thought it was best to stay till my book was ready to be sent you; for you will find in it the principles explained by which I found these and such Series. Indeed I might have sent you my Book somewhat sooner, but unluckily I was taken up with an affair which obliged me far against my inclination to defer my answer till this time.

As to M<sup>r</sup> Machin's Treatise it was written in great hurry and designed only to shew what may be expected from his larger Treatise on that Subject & therefore it is no great wonder if you met with some difficulties in it, especially considering that not only his propositions but also the principles from which most of them are deduced are new. I have prevailed on him to write an answer to that part of your Letter which relates to himself, which I now send you and hope it will satisfy you intirely till you shall see the Book he is now preparing for the press, which I am Confident will please you extremely, as it clears up the Obscure parts of Newton's third Book of principles, and carries the Theory of Gravity further than even Sir Isaac himself did. And it is somewhat strange that altho the principles have been published above 40 years, that no body has read further than the two first Books, altho they be barely Speculative and were written for no other reason but that the third might be understood.

The Theoreme which M<sup>r</sup> Cramer sent you for Interpoling by Quadratures may as you observe be deduced from one

which you sent to Mr Mommort 15 years ago, and so may it as easily be deduced from a more simple one which Dr Wallis published 75 years ago namely that  $\frac{1}{n} x^n$  is the Area of a Curve, whose Ordinate is  $x^{n-1}$  and I value it so much the more because the Demonstration of it is so very easy. But neither your Theoreme nor that of Dr Wallis is sufficient except in that case when the Series is so simple as to admit of Interpolation by a Binomial Curve, for if a Trinomial or more Compound Curve be required we must have recourse to the Comparing of Curves according to the 7 & 8 Propositions of Newton's Quadraturus, that being the generall principle for this kind of Interpolation.

I agree with you that the Series  $A, \frac{r}{p} A, \frac{r+b}{p+b} B, \frac{r+2b}{p+2b} C,$  &c. may be Interpoled without Quadratures, as you will see by many Examples in the 21, 22, 26, & 28 Propositions of my Book: but I am still at a loss to find out that it is to be done after the manner you propose by putting

Indeed it is true that the Terms may be expressed by a Fraction, but to what purpose I know not; for if the Term required be an Intermediate one, both the Numerator and Denominator of the Fraction will consist of an Infinite number of Factors, and therefore that is no Solution, for it is as Difficult, nay it is the very same Probleme, to find the Value of such a Fraction as to find the Value of the Term proposed. The fraction no more gives the value of a Term whose place is assigned, than the place of a Term being assigned gives the Fraction. Besides, that Method would not even give a primary Term which stands at a great distance from the begining of the Series: for the Number of Factors, tho not infinite, yet would be so great as to render the work altogether impracticable.

But here I except the case where the difference betwixt  $p$  &  $r$  is not much greater than  $b$ , and at the same time is a multiple of it; this is the only case when your Method will do, as far as I understand it; but when this happens, the Series is interpoleable by the bare inspection of the Factors, even without the help of common algebra: and therefore

I hope you did not imagine that I designed to trouble a Gentleman of Mr Cramer's abilities with such a simple Question, or that I pretended to reduce it to Quadratures, altho perhaps I might take it for an Example of the general solution.

I cannot but think that one of us has misunderstood the other, and therefore I should be glad to have your Method explained to me: for instance in the Series  $1, \frac{2}{1}A, \frac{4}{3}B, \frac{6}{5}C, \frac{8}{7}D,$  &c. which is the Simplest of all those which do not admit of an exact interpolation: how do you find out that the Term which stands in the middle betwixt the first & second is equal to the number 1.570796 &c? You know I find it to be such from the method of Quadratures, which demonstrates it to be double the area of a Circle whose Diameter is Unity. And how doth your method give a Term remote from the beginning; for instance the product of a million of these Fractions  $\frac{2}{1} \times \frac{4}{3} \times \frac{6}{5} \times \frac{8}{7} \times \frac{10}{9} \times \dots \frac{2000000}{1999999}$  which I can find in the quarter of an hour to be the number 1772.4540724, as you may try by the Series which was sent you for finding the proportion which the middle Uncia in the Binomial has to the Sum of all the Unciae of the same Power.

Altho you are pleased to say that you have not spent much time on Mathematicks of late, it would rather seem to be otherwise from the ingenious Problems which you mention; for my part, as their Solution depends not on new principles, and since I know not for what design they were proposed, I have not thought about them especially since you say you have solved them already. Mr Klingenstierna shew'd me a Construction of the Probleme about two fires different from yours and Extremely Simple. He has also constructed the Probleme about a Curve revolving about a point, and whereas you have said without any limitation that you found both the Curves to be algebraical, he observes that it is so only when the Areas mentioned in the Probleme are to one another as one number is to another. He has also solv'd the Probleme about a Body falling down in a Curve, and afterwards rising either in another or in the same continued; of which last you say *videtur esse res altioris indaginis*:

And as to the Probleme about finding the Latitude of the place & declination of a Star from having three altitudes of it,

and the times betwixt them, it is evident at first sight how it may be brought to an equation.

Mr Klängenstierna had shewed me that part of your Demonstration of Cotes's Theoreme which you had ready when he left you; and Mr Cramer sent me the same with the remaining part which you sent to him about the begining of this Year: indeed I take it to be an elegant Demonstration and far Superior to that of the person you mentioned. But I suppose you know that Mr De Moivre found out his Demonstration of the same Theoreme very soon after Mr Cotes's Book was published, which is now many years ago, and I am of opinion that it will please you, as it requires no Computation.

And now I come to beg pardon for this long Letter and to assure you that I am with the greatest respect

Sir

Your most obedient

most humble Servant

JAMES STIRLING.

(4)

*Bernoulli to Stirling, 1733*

Viro Clarissimo Jacobo Stirling Nicolaus Bernoulli  
S.P.D.

Epistolam tuam die 30 Septembris 1730, scriptam una cum inclusa D<sup>m</sup> Machin et cum eximio tuo (pro quo debitas ago gratias) Tractatu de Sumatione et Interpolatione Serierum Infinitarum post annum fere accepi eo tempore, quo novae Stationi in nostra Academia Professioni nempe Juris admotus variisque occupationibus implicitus fui, quae me ex illo tempore à rerum Mathematicarum studio abduxerunt, et ab attentâ et seria lectione Libri tui avocarunt. Est et alia dilatae responsionis causa. Perdideram epistolam tuam inter Schedas meas latentem, eamque multoties frustra quaesitam non nisi ante paucos dies inveni. Ignosce quaeso tam diuturnae morae. Alacrior quoque ad respondendum fuisset, si quaedam à me dicta, quae tamen nunc sub silentio praetereo, paulo aequiori animo à te et à D<sup>no</sup> Machin excepta fuissent.

Quae D<sup>nus</sup> Machin regessit contra objectionem meam circa definitionem loci, ex quo Planetæ motus maxime uniformis apparet verissima sunt. Fateor me non attendisse ad motum medium aut ad motum retrogradum Planetæ, sed studio id feci. Ego nunquam credidi Planetæ motum apparere magis regularem aut magis uniformem eo ex loco, ex quo motus in tribus orbitæ punctis æqualis apparet quam eo ex loco, ex quo motus in duobus tantum orbitæ punctis æqualis apparet, id est, motum ex primo loco apparentem minus differre à motu medio, quam motus ex secundo loco apparens. Objectio mea erat tantum argumentum, ut vocant, ad hominem. Credebam D<sup>nūm</sup> Machin estimasse regularitatem vel uniformitatem motus ex eo quod Planeta in utraque apside ex centro æquantis visus æque velox appareat; et ad hoc credendum me induxerunt hæc verba pag. 42. 'The said center *F* will be the place about which the body will appear to have the most uniform motion. For in this case the point *F* will be in the middle of the figure *LpD* (which is the equant for the motion about that point). *So that the body will appear to move about the center F, as swift when it is in its slowest motion in the remoter apsis A, as it does when it is in its swiftest motion in the nearest apsis P*' quæ verba sane alium sensum fundere videntur, quam sequentia quæ habet in sua responsione: 'I did not conclude this to be the place of most uniform motion, because it is a place that reduces the velocity in two or three or more points to an equality, but because the motion throughout the revolution differs the least possible from the mean motion.'

Obscuritatem verborum pag. 41. ubi D<sup>nus</sup> Machin demonstrat æqualitatem arearum *FpL* et *SRA* quæ ansam præbuit suspicandi paralogismum, non puto natam esse ex praeli errato, sed ex festinatione, quam ipse Auctor se adhibuisse dicit; si quidem non solum particula *and* cum in locum, quem dicit Auctor, transponenda est, sed delenda etiam particula sequens *therefore*, ego in meo exemplari locum sic correxi Pag. 40. lin. pen. pro *areas* scripsi *fluxions of the areas Lp and AFR*. Pag. 41. lin. 4. pro *the areas* scripsi *and the fluxions of the areas ASR and AFR* Pag. 41. lin. 8 deleui *And therefore* Ead. lin. post *area* adjunxi *Lp*. Ead. pag. lin. 10. pro *that* scripsi *the area ASR*.

Vehementer cupio videre, quomodo theorema tuum pro interpolatione Seriei  $A, \frac{r}{p} A, \frac{r+1}{p+1} B, \frac{r+2}{p+2} C$  &c aequae facile (quod te per jocum dixisse puto) deducatur ex theoremate Wallisii ante 75 annos publicato, quod nempe  $\frac{1}{n} x^n$  sit Area Curvae cujus ordinata est  $x^{n-1}$  ac ex isto meo theoremate quod me ante 15 annos Monmortio misisse scripseram, nimirum quod

$$\frac{1 \cdot 2 \cdot 3 \cdot 4 \dots n \times b^n}{a \cdot a+b \cdot a+2b \dots a+nb} = \frac{1}{a} - \frac{n}{a+b} + \frac{n \cdot n-1}{1 \cdot 2 \cdot a+2b} - \frac{n \cdot n-1 \cdot n-2}{1 \cdot 2 \cdot 3 \cdot a+3b} + \&c$$

Sane cum haec Series sit aequalis areae curvae cujus ordinata est  $x^{a-1} \times \overline{1-xb}^n$  in casu  $x=1$ , sola substitutione terminorum à te adhibitorum res immediate conficitur. Nam si fingamus duas Curvas, unam cujus ordinata est  $x^{r-1} \times \overline{1-x}^{p-r-1}$ , alteram cujus ordinata est  $x^{z+r-1} \times \overline{1-x}^{p-r-1}$ , faciendo  $b=1$ ,  $a=r$  et  $=z+r$ ,  $n=p-r-1$  erunt istarum Curvarum Areae per theorema meum

$$\frac{1 \cdot 2 \cdot 3 \cdot 4 \dots p-r-1}{r \cdot r+1 \cdot r+2 \dots r+p-r-1} \quad \text{et} \quad \frac{1 \cdot 2 \cdot 3 \cdot 4 \dots p-r-1}{r+z \cdot r+z+1 \dots r+z+p-r-1},$$

adeoque prima ad secundam ut 1 ad

$$\frac{r \cdot r+1 \dots r+p-r-1}{r+z \cdot r+z+1 \dots r+p-r-1} \quad \text{sive ad} \quad \frac{r \cdot r+1 \dots r+z-1}{p \cdot p+1 \dots p+z-1},$$

id est, ut primus Seriei interpolandae terminus ad alium cujus distantia à primo  $=z$ . Demonstratio haec ubique supponit idipsum alterum theorema quod allegasti, nempe

quod  $\frac{1}{n} x^n =$  areae Curvae cujus ordinata est  $x^{n-1}$  (theorema melius notum ex methodo fluxionum quam ex Arithmetica Infinitorum Wallisii) quomodo enim potuissem dicere Seriem  $\frac{1}{a} - \frac{n}{a+b} + \&c.$ , esse aream curvae cujus ordinata est  $x^{a-1} \times \overline{1-xb}^n$  in casu  $x=1$ , nisi scivissem modum eruendi areas ex datis ordinatis? Sed hoc ipsum alterum theorema solum neutiquam sufficiens est etiam in istis Seriebus quae

admittunt interpolationem per curvas binomiales. Simili modo potuisses dicere difficillima theoremata Newtoni et aliorum de quadraturis ex dicto Wallisii facili deduci posse. Quae dixisti de interpolationibus quae requirunt Curvas trinomiales aut magis compositas, quod nempe recurrendum sit ad 7 et 8 Prop. Newt. de Quadraturis, ea non magis tangunt meum quam tuum theorema; mihi animus non fuit tractatum scribere de interpolationibus, aut meum theorema pro generali interpolationum remedio venditare, sed tantum tuum à Dnō Cramero mihi missum theorema demonstrare.

Quod attinet ad alterum modum interpolandi Seriem  $A, \frac{r}{p}A, \frac{r+b}{p+b}B, \frac{r+2b}{p+2b}C, \&c.$  qui consistit in ponendo

$$\frac{r \cdot r+b \cdot r+2b \dots r+zb-b}{p \cdot p+b \cdot p+2b \dots p+zb-b} = \frac{r \cdot r+b \cdot r+2b \dots p-b}{r+zb \cdot r+zb+b \dots zb+p-b}$$

vel  $= \frac{p+zb \cdot p+zb+b \dots zb+r-b}{p \cdot p+b \cdot p+2b \dots r-b}$ , fateor illum non succedere

nisi iis in casibus, ubi differentia inter  $p$  et  $r$  est divisibilis per  $b$ , et simul numerus non admodum magnus, quod ultimum in praecedentibus meis literis ipse jam agnovi. Fateor praeterea sensum theorematis tui non recte intellexisse, credebam

enim in hac Serie  $A, \frac{r}{p}A, \frac{r+1}{p+1}B, \frac{r+2}{p+2}C, \&c.$  quam Dnūs

Cramer tanquam formulam generalem, non tanquam exemplum alius generalioris mihi miserat,  $p$  et  $r$  significare numeros integros; unde non capiebam cur haec Series, utpote quae accurate posset interpolari, ad quadraturas reduceretur. Sed his majora te praestitisse vidi cum voluptate in tuo libro, cujus Propositio 18 continet, ni fallor, idipsum quod ego per modo dictum alterum interpolandi modum monere volebam. In exemplo 1. Prop. 25. ubi tradis interpolationem unciae binomii ad dignitatem indeterminatam elevati, inveni theorema non multum absinile praedicto meo theoremati. Si fractionis

$\frac{1 \cdot 2 \cdot 3 \cdot 4 \dots n \times b^n}{a \cdot a+b \cdot a+2b \dots a+nb}$  numerator dividatur per  $b^n$ , et singuli factores denominatoris excepto primo per  $b$ , et ipsa fractio multiplicetur per primum factorem  $a$ , proveniet reciprocus terminus unciae ordine  $n+1$  in binomio ad dignitatem  $\frac{a}{b} + n$

elevato; hinc per theorema meum, ut Area ordinatae  $x^{a-1} \times \overline{1-x^b}^n$  ad  $\frac{1}{a}$ , ita unitas ad dictam unciam. Ex. gr. si ponatur  $a = 5$ ,  $b = 1$ ,  $n = 4$  erit area ordinatae  $x^4 \times \overline{1-x}^4$ , id est,  $\frac{1}{5} - \frac{4}{6} + \frac{6}{7} - \frac{4}{8} + \frac{1}{9}$  sive  $\frac{1}{630}$  ad  $\frac{1}{5}$  ut 1 ad 126 unciam termini quinti in dignitate nona. Si  $a = 1$ ,  $b = 2$ ,  $n = \frac{1}{2}$ , erit area ordinatae  $x^0 \times \overline{1-xx}^{\frac{1}{2}}$ , id est, quadrans circuli cujus radius = 1, sive area circuli cujus diameter = 1, ad 1 sive ad quadratum circumscriptum, ut unitas ad terminum Wallisii  $\square$  interponendem inter primum et secundum terminum Seriei 1, 2, 6, 20, 70, &c quae continet uncias medias dignitatum parium, sive ad terminum qui consistit in medio inter duas uncias 1 et 1 in potestate simplici binomii; sicut tu quoque invenisti in exemp. 2. dictae Prop. 25.

Laboriosa quidem sed elegans est methodus per quam invenisti ope Logarithmorum interpolationem Seriei 1, 1, 2, 6, 24, 120, &c in Ex 2. Prop. 21. Ceterum frustra quaesivi modum, quem dixisti in sequentibus monstrari, interpolandi hujusmodi Series absque Logarithmis, quod autem à te praestare posse nullus dubito. Terminum qui consistit in medio inter duos primos 1 et 1 ope Theorematis mei sic eruo. Sit in dicto theor.  $a = n + 1$ ,  $b = 1$ , eritque area ordinatae

$$x^n \times \overline{1-x}^n = \frac{1.2.3.4 \dots n}{n+1. n+2 \dots 2n+1} = \frac{1.2.3.4 \dots n \times 1.2.3 \dots n}{1.2.3 \dots 2n+1}$$

Fiat  $n = \frac{1}{2}$  eritque area ordinatae  $\sqrt{x-xx}$  i.e. area semicirculi, cujus diameter = 1, aequalis dimidio quadrato quaesiti termini. Hinc quoque deducitur interpolatio terminorum intermediorum in hac Serie 1, 1, 3, 15, 105, 945, &c. Nam si fiat  $a = 1$ ,  $b = 2$ ,

erit area ordinatae  $x^0 \times \overline{1-xx}^n = \frac{1.2.3 \dots n \times 2^n}{1.3.5 \dots 1+2n}$ ; sed in

casu  $n = \frac{1}{2}$  praedicta area sit aequalis areae circuli cujus diameter = 1, et numerator fractionis sit aequalis radici quadratae duplae istius areae, per modo ostensa, denominator autem fractionis sit aequalis termino qui consistit in medio inter secundum et tertium Seriei 1, 1, 3, 15, 105, 945, &c proinde ut radix quadrata dimidiae areae circuli ad 1, ita unitas ad terminum illum intermedium, qui per binarium divisus dabit medium inter duos primos 1 et 1 dictae Seriei.

De modo inveniendi radicem aequationis fluxionalis per Seriem infinitam, de quo agis in Scholio Prop. ult. Part. I. etiam ego aliquoties cogitavi, et hac de re scriptum aliquod communicavi cum D<sup>no</sup> de Maupertuis cum apud nos ageret, in quo sequentia observavi. Posse inveniri Series generaliores quam quae inveniuntur per parallelogramum Newtoni; non necesse esse ut indices dignitatum in terminis Seriei quaesitae aut aequationis transformatae cadant in eandem progressionem arithmeticam; posse aliquos indices esse irracionales; et propterea tam Taylori regulam in Prop 9 quam tuam in Enumerat. Linear. tertii ordinis datam, pro determinanda forma Seriei fallere; posse per terminos solitarios in aequatione transformata nonnunquam aliquid determinari, absque ut omnes coefficientes fiant aequales nihilo; non necesse esse, ut Serierum in aequatione transformata provenientium ad minimum duorum terminorum primorum indices inter se aequentur, ut determinetur coefficiens primus  $A$ , quia hic nonnunquam potest ad arbitrium assumi; posse evitari terminos superfluos, quorum coefficientes in methodo Taylori evadentes  $= 0$  laborem calculi prolixiorem reddunt, quam parat. Sic pro Exemplo Taylori in Prop. 9. Method. Incr. pag. 31  $1 + xz - z^{\frac{3}{2}}xz - \dot{x} = 0$  sequentes 4 Series inveni; quarum tres priores sunt generaliores illis quas Taylorus invenit.

$$1^a. x = A + Bz + \frac{1}{2}z^2 + \frac{1}{6}Az^3 - \frac{4}{35}ABz^{\frac{7}{2}} + \frac{1}{12}Bz^4 - \frac{4}{63}\overline{AA + BB}z^{\frac{9}{2}} + \frac{1}{40}z^5 \&c.$$

$$2^a. x = \frac{7}{2}z^{-\frac{5}{2}} - \frac{14}{29}z^{\frac{1}{2}} + Cz \frac{-5 + \sqrt{165}}{4} + 4z^2 - \frac{288}{5887}z^{\frac{7}{2}} \&c$$

$$3^a. x = 2z^{\frac{1}{2}} + B - \frac{1}{2}z^{-1} + \frac{1}{6}Bz^{-\frac{3}{2}} - \frac{1}{16}BBz^{-2} + \frac{1}{40}B^3 - \frac{3}{20}z^{-\frac{5}{2}} \&c$$

$$4^a. x = -z^{-1} - z^{-\frac{5}{2}} - \frac{1}{2}z^{-1} - \frac{1}{4}z^{-\frac{1}{2}} \&c.$$

Sic quoque observavi te non satis accurate rem examinasse, quando pag. 83 dicis, aequationem  $r^2\dot{y}^2 = r^2\dot{x}^2 - \dot{x}^2y^2$  nulla alia radice explicabilem esse praeter duas exhibitas

$$y = r - \frac{x^3}{6r^2} + \frac{x^5}{120r^4} - \frac{r^7}{5040r^6} + \&c \text{ et}$$

$$y = A \times 1 - \frac{x^2}{r^2} + \frac{x^4}{24r^4} - \frac{x^6}{720r^6} + \&c$$

quarum prior dat sinum, et posterior cosinum ex dato arcu  $x$ ; et de qua posteriore dicis, quantitatem  $A$  quae aequalis est radio  $r$  ex aequatione fluxionali non determinari. Ego non solum inveni, Seriem non posse habere hanc formam  $A + Bx^2 + Cx^4 + Dx^6$  &c nisi fiat  $A = r$ , sed utramque à te exhibitam Seriem comprehendi sub alia generaliori, quae haec est:  $y = A + Bx + Cxx + Dx^3 + Ex^4 +$  &c in qua coefficientes  $A, B, C, D$  &c hanc sequuntur relationem

$$BB = \frac{rr - AA}{rr}, \quad C = -\frac{A}{1 \cdot 2 \cdot rr}, \quad D = -\frac{B}{2 \cdot 3 \cdot rr},$$

$$E = -\frac{C}{3 \cdot 4 \cdot rr}, \quad F = -\frac{D}{4 \cdot 5 \cdot rr} \text{ \&c}$$

Si fiat  $A = 0$ , habetur Series pro Sinu; sin autem  $A$  fiat  $= r$ , habetur Series pro cosinu; sin vero  $A$  alium habeat valorem praeter hos duos, etiam alia Series praeter duas exhibitas erit radix aequationis fluxionalis propositae. Similiter Series illae quatuor, quas exhibes pag. 84. pro radice aequationis  $\ddot{y} + a^2 y - x\dot{y} - x^2 \ddot{y} = 0$ , sub aliis duabus generalioribus quae ex tuis particularibus compositae sunt, comprehenduntur. Duae nempe priores sub hac

$$y = A + Bx + Cxx + Dx^3 + Ex^4 + \text{\&c.}$$

in qua coefficientes  $A$  et  $B$  habent valores arbitrarios, reliqui autem  $C, D, E$ , &c sequentem ad priores habent relationem

$$C = \frac{0 - aa}{1 \cdot 2} A, \quad D = \frac{1 - aa}{2 \cdot 3} B, \quad E = \frac{4 - aa}{3 \cdot 4} C, \quad F = \frac{9 - aa}{4 \cdot 5} D \text{ \&c.}$$

Si  $B = 0$  habetur tuarum Serierum prima, Si  $A = 0$  habetur secunda. Duae posteriores comprehenduntur sub hac generali forma  $y = Ax^a + Bx^{-a} + Cx^{a-2} + Dx^{-a-2} + Ex^{a-4} + Fx^{-a-4} +$  &c. ubi iterum  $A$  et  $B$  habent valores arbitrarios,

$$C = -\frac{a \cdot a - 1}{4 \cdot a - 1} A, \quad E = -\frac{a - 2 \cdot a - 3}{8 \cdot a - 2} C,$$

$$G = -\frac{a - 4 \cdot a - 5}{12 \cdot a - 3} E, \text{ \&c; } D = \frac{a \cdot a + 1}{4 \cdot a + 1} B,$$

$$F = \frac{a + 2 \cdot a + 3}{8 \cdot a + 2} D, \quad H = \frac{a + 4 \cdot a + 5}{12 \cdot a + 3} F, \text{ \&c.}$$

Si fiat  $B = 0$  exsurgit tua tertia Series, et si fiat  $A = 0$  exsurgit quarta, in qua termini per signum  $+$  non per signum  $-$  connecti debent. Incomodum quoque est in tuis Seriebus, quod Literae  $A, B, C, D$  &c mox pro coefficientibus terminorum, mox pro ipsis terminis usurpentur.

Hac data occasione describam hic ea quae ad quasdam tuas Series in Libro tuo de Enumeratione Linearum tertii ordinis contentas notaveram eo tempore, quo hunc Librum a Dñō de Maupertuis comodatum habebam. Eum quidem nunc non habeo, sed in quadam mea Scheda haec notata reperio. In *Exemplo 2 pag. 22.* aequationis  $x^3\dot{y} + ayx\dot{x} + a^2x\dot{x} - 2a^3\dot{x} = 0$  radix  $y$  est =

$$A + \frac{aA + aa}{x} + \frac{aaA - a^3}{2xx} + \frac{a^3A - a^4}{2 \cdot 3 \cdot x^3} + \frac{a^4A - a^5}{2 \cdot 3 \cdot 4x^4} + \&c$$

quando  $A = 0$  provenit Stirlingii solutio; sed quando  $A = a$  exsurgit  $y = a + \frac{2aa}{x}$ .

In *Ex. 4. pag. 26.*  $y^2\dot{x}^2 - 3x^2\dot{x}\dot{y} + 2x^2\dot{x}^2 - ax\dot{y}^2 + a^2\dot{x}^2 = 0$  radix  $y$  est =  $x + Bx^{\frac{1}{3}} - BBa^{\frac{1}{3}} + \frac{a}{2} + B^3x^0 + \frac{1}{9}aB - B^4 \cdot x^{-\frac{1}{3}}$

$$+ \frac{5}{36}aBB + B^5 \cdot x^{-\frac{2}{3}} - \frac{aa}{4} - \frac{3}{10}aB^3 - B^6 \cdot x^{-1} \\ + \frac{17}{324}aaB + \frac{73}{180}aB^1 + B^7 \cdot x^{-\frac{4}{3}} \&c$$

item  $y = 2x + a - \frac{2aa}{7}x^{-1} + \frac{6a^3}{35}x^{-2} - \frac{88a^4}{637}x^{-3} \&c.$

*Pag. 28.* aequationis  $y^3 - ay^2 + a^2y - a^3 + x^2y = 0$  radix  $y$  non est =  $a + \frac{x^2}{2a} - \frac{x^4}{2a^3} \&c$  sed  $a - \frac{x^2}{2a} + \frac{x^6}{16a^5} + \frac{x^8}{32a^7} \&c.$

*Pag. 31.*  $y = x \pm \frac{aa}{\sqrt{ax}} - \frac{a^3}{2x^2} \pm \frac{5a^5}{8x^3\sqrt{ax}} \&c.$

et  $y = \frac{a^3}{x^2} + \frac{2a^6}{x^1} + \frac{7a^9}{x^8} \&c.$  vid. pag. 128.

*Pag. 34 Ex. 1.* Satisfacit etiam

$$y = x - \frac{x^3}{aa} + \frac{x^4}{a^3} - \frac{4x^6}{a^5} + \frac{4x^7}{a^6} \&c.$$

In eadem Scheda notatam reperio Speciem aliquam linearum tertii ordinis à te et à Newtono omissam. Nempe in Libri tui pag. 112. Sp. 58 ubi pro *aequales et ejusdem signi* legi debet *aequales affirmativae*; nam si radices sint aequales negativae, figura non evadit cruciformis, sed habet crura ut in fig. 57. et praeterea punctum conjugatum in diametro  $AB$ , quod reperitur faciendo abscissam  $= -\frac{4}{2b}$ .

It igitur haec nova Species est diversa à Specie 53 Newtoni, apud quem in mentione Speciei 54 pro *impossibiles* etiam legi debet *aequales affirmativae*.

Problemata de quibus in fine epistolae meae mentionem injeci, cum in finem subjunxi, ut petitioni tuae aliquo modo obedirem impertiendo nova quaedam Mathematica. Mos iste Problemata proponendi et alios ad eorum solutionem amice invitandi, non est omnino culpandus, si is nempe scopus propositionis sit, ut communicatis invicem methodis solutionum Ars Analytica incrementum capiat. Dietorum Problematum solutiones Patruus meus et ego cum Dño Klingenstierna tum apud nos degente communicavimus; hinc credo constructionem quam hic tibi ostendit, Problematis de Cûrva recessus intra duos ignes, et quam tamquam valde simplicem laudas, non aliam esse quam Patruï mei, qui hoc Problema ope Trajectoriarum Orthogonalium ingeniose quidem solvit, sed ipsius trajectoriae orthogonalis sive curvae quaesitae constructionem non dedit. De problemate circa curvam circa datum punctum revolvantem recte monitum est utranque curvam esse algebraicam; si areae de quibus in Problemate sermo est, sint ut numerus ad numerum; haec limitatio tanquam facile animavertenda à me studio omissa fuit. Vix est ut credam Problema in Act. Lips. 1728. pag. 523 propositum à d. Klingenstierna solutum fuisse eo etiam in casu, de quo dixi, videri rem esse altioris indaginis.

Moivraei demonstratio Theorematis Cotesiani sive resolutio fractionis  $\frac{1}{z^{2n} - 2tz^n + 1}$  in fractiones simpliciores habentes denominatores duarum dimensionum mihi perplacet, quamvis ob concisum sermonem explicatione quadam opus habeat, Posteriorem partem demonstrationis meae, quam ex cōmunicatione Dñi Crameri vidisti ab inductionis vitio liberavi

substituta liquida et rigida demonstratione, quam ad eundem D. Cramerum Amicum nostrum mitto in epistola cui hanc ad te perferendam includo. Vale.

Dab. Basileae d. 1. Aprilis. 1733.

P.S. Nescio quo fato acciderit ut nomen meum in Catalogo Sociorum R. S. omissum sit. Conjicio id factum esse hae ratione; primum nomen meum mutatum fuisse in nomen Adgnati mei Nic. Bern. Professoris tum Bernensis, postea Petroburgensis; dein ex catalogo expunctum post hujus obitum. Spero hunc errorem emendatum iri.

## V

## CASTEL AND STIRLING

(1)

*Castel to Stirling, 1733*

Doctissime Vir

LIBENTER vidi quae de me in epistola ad clarissimum amicum D. de Ramsay scripsisti, et gratias pro benevolentia tua in me habeo quam plurimas. Jamdudum professus sum quanti sit apud me. Vidisti haud dubie quae in commentariis Trivoltiensibus scripsi circa opusculum tuum ultimum de seriebus infinitis tum summandis tum interpolandis.

Quod nunc attinet ad aequabilitatem arearum Newtonianam, nollem mihi tribuisses errorem adeo crassum quasi lineam eandem duobus aliis non parallelis parallelam afficerem. Vel ipsa verba mea reclamant, licet verbis figura non satis respondet. Supposui enim statim cum Newtono lineolam  $Cc$  parallelam  $SB$ ; et deinde distincte supposui lineolam aliam  $CR$  parallelam  $BT$ . Relegere potes haec ipsissima verba mea pag. 539 (et tirant  $CR$  parallele a  $BT$ ) quae si advertittas aliter profecto rem accepisses, neque demonstrationis meae errorem sed demonstrationis Newtonianae vitium deprehendisses; vitium dico non quidem geometricum sed physicum, quod plerisque summi illius geometrae demonstrationibus accidit, quae quidem geometricae verae sunt, a veritate physica autem omnino aberrant. Sensus itaque demonstrationis meae iste est.

Suppono constructionem et demonstrationem Newtonianam circa punctum  $S$ .

En meum circa punctum  $T$ . Duco  $CR$  parallelam  $TB$ , et dico  $ATB = BTC$ , atque  $BTC = BTR$ . Ergo quod erat demonstrandum. Tam vera est haec demonstratio quam

Demonstratio Newtonii et si quidquid circa illam dixi paginis totis 531, 32. 33. 34. 35. 36. 37. 38. 39. 40. 41, dignatus esses legere, sensisses non in toto trium linearum non parallelarum

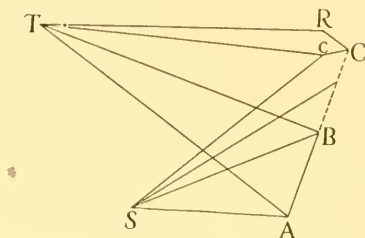


FIG. 28.

parallelismo rem stare, sed in ipsa praeipue curvarum geometricarum natura, quarum latera infinitesimalia sunt omnino indeterminata ut hoc vel illo modo physico resolvantur in determinationes laterales numero infinitas.

Conclusio autem tua, non est mea, quam mihi affingis. Non sequitur ex mea demonstratione sectores  $AED$ ,  $DEB$  quos satis scio esse inaequales, esse aequales. A finito ad infinitum, ab infinitesimali ad finitum non valet consequentia. Diversa elementa, diversae fluxiones dant fluentes omnino diversas. In priori figura  $CB$  non est  $= RB$ , nec fortasse

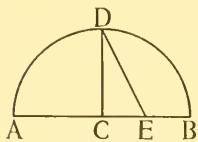


FIG. 29.

$TAB = SAB$ . Diversae sunt etiam vires centripetae  $Cc$ ,  $CR$ .

Vera est autem observatio Kepleri vera est Demonstratio Newtoni: sed non vere ista demonstratio huic observationi applicatur: vel potius vera est utraque inclusiva non autem exclusiva. Punctum  $S$  centrum esse hinc ita demonstratur; ut centrum sit et  $T$  eodem modo, et quodvis punctum aliud, nullum enim est ad quod non dirigatur vis centripeta ut ipse adstruit Newtonus, varias versus varia puncta curvae definiens vires centripetas.

Excidit mihi superius plerasque Newtoni assertiones geometricae veras, physice falsas esse. Parce vir doctissime huic ingenuitati meae, admiror Newtonum nullum novi geometram illi antefendum. Physicae vitium est: nimis geometricae tractari renuit, quamvis tota sit geometrica, natura, ut ajunt, geometrizat semper: sed geometria sese infinitis accommodat

hypothesibus; nec quidquid geometricum est, continuo physicum esse convincitur. geometria circa abstracta versatur, circa possibilia, possibilia autem sunt numero infinita: unicum est in quolibet phenomeno naturae systema: nec a possibili ad actum valet consequentia.

A quindecim circiter annis opusculum composui quo physicum Newtoni convellere totum mihi videbar. Praelo paratum erat opus; summa mea Newtoni reverentia cohibuit ne publice illud juris facerem: nec faciam credo equidem tanta in animo meo insidet summi illius viri existimatio. Vale vir clarissime, meque tui observantissimum, servumque humillimum habe.

LUDOVIC CASTEL.

Parisiis die 25 Martiis 1733

P.S.

Status quaestionis est. vult Newtonus aequabilitatem arearum aequabili tempore descriptarum signum esse certis-

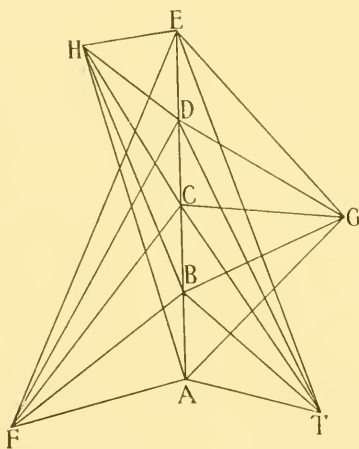


FIG. 30.

simum, proprium, unicum centri respectu ejus ea regnat aequabilitas. contendo ego signum illud esse omnino aequivocum. nec unam hac de re demonstrationem assero unicam impugnas clarissime vir. omnes sunt impugnandae si assertionem Newtonianam salvam velis. nam vel ea quae circa hic appositam figuram versatur totum systema Newtoni convellit: demonstrat enim 1°. sine ulla vi centripeta, et sine ullo centro

aequabiles esse tamen areas circa punctum  $E$ . 2<sup>a</sup>. infinita esse puncta circa quae haec vigeat aequabilitas. atque enim curvis eidem obtinet indeterminato. ruit ergo propositio haec fundamentalis Newtoniani systematis.

(2)

*Stirling to Castel*

Reverendo Patri D<sup>o</sup> Ludovico Castel.

Doctissime Celeberrimeque Vir

Gratias ago maximas propter epistolam quam nuper ad me scribere dignatus es, cui certe responsum antehac dedissem, si per varia negotia licuisset. Commentaria trivoltiensia ad manus meas nondum pervenere, fateor tamen me pluribus nominibus tibi devinctum propter ea quae in aliis tuis operibus de me scripta videram. Cur ego ad amicum communem D. Ramsay ea scripsi quae tibi paulo liberius videntur, in causa fuit tua erga me publice attestata benevolentia, quam certe credebam me satis remunerari non posse, agnoscendo librum tuum de gravitate esse multiplici eruditione refertum si non libere etiam tecum communicarem objectiones quasdam mea opinione haud male fundatas; hoc enim ni fallor non minus quam illud munus est amici.

Quantum ad aequalitatem arearum circa centrum virium, ego in pagina 539 tui libri credebam  $CR$  fuisse errorem praeli,

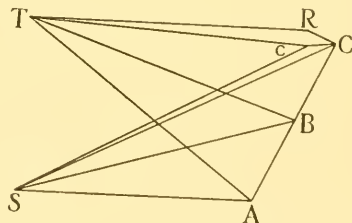


FIG. 31.

si quidem nulla istius modi linea extat in schemate; et pro eadem legebam  $Cc$ . Et procul dubio oportet  $CR$  et  $Cc$  esse unam atque eandem tam magnitudine quam positione nisi fingas duas esse vires centripetas ut in tua epistola. Ibi supponis demonstrationem Newtoni pro aequalitate arearum circa punctum  $S$ , dein profers propriam pro arcibus circa punctum

$T$ , quam ais tam veram esse quam eam Newtoni; quod ego libenter concedo. Nam si existente  $S$  centro virium areae circa idem aequales sint per demonstrationem Newtoni; annon per eandem demonstrationem areae erunt aequales circa aliud quodvis punctum  $T$  modo idem supponatur esse centrum virium? Sed quid hoc ad nostram controversiam ego sane nondum percipio. Tuum est demonstrare areas esse aequales circa punctum quod non est centrum virium, alias inconcussa manebit veritas propositionis Newtonianae.

Inquis me si perlegerem paginas 531, 532 &c 'sensurum non in solo trium linearum parallelarum parallelismo rem stare, sed in ipsa praecipue curvarum geometricarum natura, quarum latera infinitesimalia sunt omnino indeterminata ut hoc vel illo modo physico resolvantur in determinationes laterales numero infinitas'. Sed post lectas sedulo paginas mihi recommendatas, minime sentio rem stare in natura curvarum, etiamsi resolvi possint in latera infinitesimalia ad libitum. Et si  $CR$  et  $Cc$  supponantur non coincidere erunt duae vires centripetae, quo in casu nihil probari potest contra Newtonum. Ut autem coincident est impossibile, quoniam  $SA$  et  $TA$  non sunt parallelae.

Revolvatur jam corpus in semicirculo  $ADB$  cujus centrum  $C$ , et  $E$  punctum quodvis in diametro  $AB$ , cui normalis sit  $CD$ . Dico impossibile esse areas circa puncta  $C$  &  $E$  descriptas esse temporibus proportionales. Sit enim si fieri potest. Itaque ex hypothesi erit ut tempus quo arcus  $AD$  describitur ad tempus quo arcus  $DB$  describitur ita quadrans

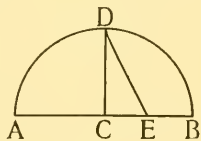


FIG. 32.

$ACD$  ad quadrantem  $DCB$ ; et eadem de causa ut tempus quo describitur arcus  $AD$  ad tempus quo describitur arcus  $DB$  ita area  $AED$  ad aream  $DEB$ ; unde ex aequo ut quadrans ad quadrantem ita sector  $AED$  ad sectorem  $DEB$ , unde ob quadrantes ejusdem circuli sibi invicem aequales, erit area  $AED$  aequalis  $DEB$ . Quod est absurdum, nam prior excedit quadrantem, posterior vero ob eadem deficit triangulo  $CDE$ . Haec autem deducitur consequentia non arguendo a finito ad infinitum aut ab infinitesimali ad finitum, sed argumentando per aequalitatem rationis.

Et in quacunque curva deferatur corpus, geometricè semper

demonstrari potest, impossibile esse ut areae circa duo puncta descriptae sint temporibus proportionales.

Ais veram esse observationem Kepleri et veram esse demonstrationem Newtoni sed non vere applicatam huic observationi quod ultimum velim ostendes. Deinde ais 'punctum *S* centrum esse ita demonstratur ut centrum sit et *T* eodem modo et quodvis punctum aliud, nullum enim est ad quod non dirigitur vis centripeta, ut ipse adstruit Newtonus, varias versus varia puncta curvae definiens vires centripetas'.

Newtonus ut demonstret vim qua planetae retinentur in orbibus tendere ad centrum Solis, ostendit per prop. 2. lib. I corpus omne quod movetur in curva, et radio ad punctum immobile ducto describit areas Temporibus proportionales, urgeri a vi centripeta tendente ad idem punctum; quumque Keplerus observasset planetas describere areas circa solem temporibus proportionales, concludit vires quibus planetae retinentur in orbibus tendere ad centrum Solis. Et haec est legitima argumentatio quoniam unicum tantum est punctum circa quod areae descriptae sunt temporibus proportionales. Unde constat nec punctum *T* nec aliud quodlibet probari posse centrum virum nisi prius observetur areas circa idem descriptas esse temporibus proportionales.

Newtonus definivit legem vis centripetae tendentis ad punctum quodvis in genere, et exinde non sequitur eum adstruere vim centripetam tendere ad omnia puncta, e contra tota vis demonstrationis propositionis 1<sup>mae</sup> Lib I de aequabilitate arearum pendet ex hoc quod vis centripeta dirigatur ad unicum punctum idque immobile. Nam si dirigerentur ad punctum mobile, vel ad duo aut plura puncta propositio esset falsa. Et si vis centripeta tenderet ad duo puncta immobilia, tum triangulum confectum lineis jungentibus puncta illa duo et centrum corporis moventis describeret solida proportionalia temporibus, ut paucis abhinc annis invenit D. Machin. Lex autem pro pluribus centris quam duobus nondum est reperta: aequalitas arearum ad unicum centrum pertinet.

Inquis plerasque Newtoni assertiones esse geometricae veras, & physicae falsas; hanc distinctionem fateor me non capere. Nam secundum me assertio geometricae vera est propositio demonstrata; haec erit semper et ubique vera, nec falsa physicae aut metaphysicae, aut alio quovis modo. Fieri quidem

potest propositionem geometricam in rerum natura locum non habere propter aliquam suppositionem quae in natura non est, sed inde non sequitur propositionem esse falsam. Exempli gratia si nulla existat linea absolute recta in rerum natura, tum nullum exstabit triangulum cujus tres anguli aequantur duobus rectis; attamen est propositio vera non solum geometricae sed et in omnibus scientiis, quod tres anguli trianguli aequantur duobus angulis rectis modo latera ejus sint lineae rectae. Si tantum velis, non sequi conclusiones geometricae inventas existere nisi per experimenta vel observationes constiterit hypotheses quibus inmituntur haec conclusiones existere, inficias non ibo.

Si habes opusculum apud te quo physica Newtoni tota convelletur, oro te meo et omnium nostratum nomine ut eundem illico mandes praelo, neve patiari Newtoni reverentiam te cohibere a propaganda veritate; cujus amor apud nos antecellit reverentiam qua colimus mortaliū quemvis.

In conclusione dicis 1<sup>mo</sup> sine ulla vi centripeta et sine ullo centro aequabiles esse tamen areas circa punctum *E*. In cujus contrarium aio demonstrationem Newtoni in eo fundari, quod sit vis centripeta continue agens, et quod vis illa semper tendat ad unicum immobile centrum. Secundo dicis infinita esse puncta circa quae haec vigeat inaequalitas; hujus autem impossibilitas geometricae demonstrari potest, de quo itaque non est mihi disputandum. Adeoque post omnia quae ad me scripsisti, non percipio propositionem fundamentalem Newtonianae systematis ruere: ignoscas interim oro si tibi assentire nequeo, et obsecro ut tu legas hanc epistolam eodem animo quo ego eandem scripseram. Quod superest valeas illustrissime Vir, meque tibi devinctissimum et obsequentissimum credas

JACOB. STIRLING

Londini Julii 1733 S.V.

## VI

### CAMPAILLA AND STIRLING

(1)

*Campailla to Stirling, 1738*

Clarissime, & Doctissime Domine

QUAM primum ad me successive pervenerunt quaedam Opera Insignis Scientiarum Antistitis, & in Mathesi longe praestantissimi Aequitis Angli Isaaci Newton votis annuente candido Amico nullam pati moram tanti Viri apud Vos illustre nomen, quin ocius ea perlustrarem fecit. Ut ut eximia me tenuerit jucunditas, dum perlegerem mathematica Philosophiae Principia, nec minus dein Opticae libros, in nonnullas incidi dubitationes, quas calamo inermi in binos includere Dialogos, lubuit. Praelo evulgare formidavi, neve mihi petulantis notam inureret, quam longe patet, Sapientum Respublica & indignationem apud Vestrates incurrerem; quod auderem censoriâ virgâ philosophicam tangere hypothesim, quam literarius Orbis eximio prosequitur honore, magnaq; reverentiâ colit. Tandem timorem ex animo prorsus excussit admodum Reverendus è Societate Jesu Pater Melchior Spedaleri, qui per Epistolam significavit, te mira, qua ornaris ingenuitate, ac candore ad Patrem Castel, hisce, quae subdo verbis scripsisse, quibus petisses, ut difficultates, quas adversus Newton haberet, typis statim mandaret, siq; talia fando, enim adhortatus fuisti: ‘Oro te, meo & omnium Nostrum nomine, ut illud praelo statim mandes: neve patiare reverentiam Newtoni plus apud te valere, quam amor Veritatis: nam certo apud Nos plus valet amor veritatis, quam reverentia, qua columus Mortalium quemvis’. Revocato igitur animo ab tui consilii heroica sinceritate, qui inter caeteros, quibus decoratur Societas Regia Londinensis Mathematicos & Philosophos, emicet cele-

berrimus, constitui nedum publice juris facere, verum modo Opusculum hocce quaecumq; meum ad te transmittere. Unum ab ingenita Humanitate tua enixe depraecor, Vir Clarissime ne dedigneris Sapientiae tuae dubia me edocere; ab te uno enim solidam accipere sententiam potero certe; eruntq; mihi & dogmata. & oracula. Calleo prorsus, ut rem pergratam, diu: praestes exoptatam haud valere famulatus mei officia; at recorderis, oportet, quos sublimiori Sapientia ditavit natura, quaecumque agenda suscipiunt, virtute propria peragere, quae sibi met sola praemia dat. Vale interim, felicissime vive, & dum te docentem habere obsecro, tuo nomini in omne aevum suscipe

Motuae die sexta Mensis Maii 1738

Addictissimum & Obsequentissimum

THOMAM CAMPAILLA

## VII

### BRADLEY AND STIRLING

(1)

*Stirling to Bradley, 1733*<sup>1</sup>

Tower-street, London, Nov. 24, 1733.

Dear Sir,

I was very sorry that I did not see you when last in town, because I wanted very much to have conversed about the experiment made in Jamaica, which I hear you have considered, as indeed I have also done. If the pendulum went slower there than here by  $2'16''$  in a sidereal day, and only  $9''$  or  $10''$  are to be allowed for the lengthening of it by heat, as Mr. Graham tells me, thence it would follow that the earth's diameters are as 189 to 190, or thereabouts, in which case the force of gravity at the equinoctial would be to the centrifugal force as  $237\frac{1}{2}$  is to unity; which is impossible, unless the diameter of the earth were above 9000 miles, and that differs so much from the measures of Norwood, Picart, and Cassini, that it cannot be admitted, nor consequently the experiment from whence it is deduced: and besides, I can prove from undoubted observations in astronomy, that Cassini's measure is very near the truth, for the diameter of the earth can be found surer by them than by any actual mensuration. If  $29''$  could be allowed for the lengthening of the pendulum by heat, this experiment made at Jamaica would agree with other things, but Mr. Graham says that he cannot allow that by any means. I am very far from thinking that the experiment was not exactly made, and indeed a greater absurdity would follow from Richer's experiment made in the island of Cayenna, which is the only one that can be depended on, which is mentioned in sir Isaac's *Principia*.

<sup>1</sup> Pp. 398-400 of *Miscell. Works & Corresp. of James Bradley*.

Although I have treated of the problem of the figure of the earth in a manner which is new, yet I am still obliged to suppose the figure of it to be an exact spheroid, and although I be sensible that this supposition is not sufficient to determine the number of vibrations to 8'' or 9'' in a day, yet I know that the error cannot be so great as the Jamaica experiment makes it. If Mr. Graham be certain that not above 10'' can be allowed for the heat, it is as certain either that the mountains have a sensible effect on the pendulum, or some other thing, which will render the experiment entirely precarious.

I find that sir Isaac in his 3d edit. Princip. mentions three observations of Dr. Pound, which make Jupiter's diameter about 37''; I want to know if that be the greatest diameter of Jupiter; because if it be, then the lesser would be about 34'', which would make too great an odds in the thing for which I want it. And I should be glad to know if you can help me to any observation which ascertains the moon's middle distance from the earth, which I could depend more on than the common ones; if you could inform me of these things, I should be able quickly to make an end of what I shall say about the figure of the earth, which I would the more willingly do, because not only Mairan, but also Hugen, Herman, and Maupertuy, have all of them entirely mistaken the matter. I heartily wish you all happiness, and the sooner I hear, the more you will oblige,

Sir, your most humble servant,

J. STIRLING.

(2)

*Bradley to Stirling, 1733*

To

Mr James Stirling  
at the Academy in Tower Street  
London

Dear Sir

When I was last in London an unexpected accident obliged me to return hither sooner than I intended; and hindred me from waiting on you, as I proposed to have done; having been informed that you were then examining into the Dispute

concerning the Figure of the Earth. Not that I had much more to tell you, than what is contain'd in the Account of the Jamaica Experiment, which I left with Mr Graham; wherein I have stated the Facts as well as I could, and made such allowance for the lengthening of the Pendulum by Heat as former Observations and Experiments would warrant.

The Result of all seem'd to be that the Clock went  $1'58''$  p Diem slower in Jamaica than at London. I allowed only  $8\frac{1}{2}''$  on account of the different degrees of Heat, having no Authority from former experience to make any greater Abatement; so that I apprehend this Retardation of the Clock (so much greater than what is derived by a Computation founded on the Principles of Gravity and an uniform Density in ye several parts of the Earth) must be rather ascribed to an inequality in the Density of the parts of ye Earth near which the Clock is fix'd, than to the greater Heat. For the greatest part of the force of Gravity upon any particular Body arising from the parts of the Earth that are near it (the Action of ye remote parts being but small) does it not thence seem likely that a Body placed near a great Quantity of rarer Matter as Water &c: will not be attracted with so much Force as if it were in the midst of a large quantity of Denser Matter, as in a great Tract of Land &c? and may it not thence follow that Clocks (tho' in the same Latitude) may yet not go alike, when placed on y<sup>e</sup> Continent and on Islands or on larger and smaller Islands? or may not the Mountains (as you observe) according as they contain Matter more or less Dense, contribute something towards such Inequalities. These considerations do at least suggest the necessity of a great variety of exact experiments made in different Places, situated in the same, as well as in different Latitudes, and I have (for this reason) proposed in the fore-mentioned Account, to have the Experiment repeated in several Places, in order to discover whether the Density of Different Regions be uniform or not; for till that Point is settled, we may be at a loss for the true cause of this Difference between the Theory & Experiment.

As to the Diameters of Jupiter, I find from the Mean of several Observations which I made with the R. Society's Glass of 123 feet focus, that the greater Diameter is to the Lesser (when both were measured with a Micrometer) as 27 to 25.

the greatest Diameter (at  $2^s$  mean Distance from y<sup>e</sup> Earth or Sun) being just  $39''$ . This is the Case when ye Diameter was actually measured with the Micrometer; but by other observations of the Time of the Passage of some of the Satellites over  $2^s$  Disk, compared with their greatest apparent Elongations taken with a Micrometer, the Diameter of  $2$  comes out only  $37''$  or  $38''$ , the difference arising (as I conceive) from y<sup>e</sup> Dilatation of Light &c.

Having never made any Observations myself particularly with a view to determine the Moons mean Distance I can give you no information relating to that Point, but believe Mr Machin has examined that matter and fix'd it with all the accuracy that the best Observations we have, would enable him to do it.

You would have had my Answer sooner, had I not been engaged in a Course &c upon y<sup>e</sup> conclusion of which I have taken the first opportunity of assuring you that I am with great Respect

Sr Your most obedient

humble Serv<sup>t</sup>

JA: BRADLEY.

Oxford }  
Dec. 2<sup>d</sup> }  
1733 }

## VIII

### KLINGENSTIERNA AND STIRLING

(1)

*Klingenstierna to Stirling, 1738*

Viro Clarissimo, Doctissi  
moque Domino  
Jacobo Stirlingio  
Londinium

at y<sup>e</sup> Academy in little  
Tower Street.

---

Clariss. Viro  
Jacobo Stirlingio  
Sam. Klingenstierna  
S. p. d.

Duplici nomine indulgentia Tua maximopere me egere sentio uno, quod multis singularis ejusdam benevolentiae documentis à te affectus per tantum temporis spatium siluerim: altero quod nunc tandem silentium rumpens non dubitaverim negotiorum nonnullorum demandatione tibi esse molestus. Sed quemadmodum Te persuasissimum esse velim, me officia & studia in me Tua, quae dum Londini agerem, multis modis expertus sum, prolixiori animi affectu quam verborum apparatu agnoscere, semperque agniturum esse: Ita spero etiam te non aegre laturum, quod Tibi amicorum optimo harumque rerum intelligentissimo ejusmodi negotia demandem, quae ad comunium studiorum quaecunque incrementum aliquid forte conferre poterunt. Constitui nimirum apparatus Instrumentorum Physicae Experimentalis inservientium quam potero perfectissimum mihi comparare. Eumque in finem instrumenta quae apud nos per peritiam artificum fabricari possunt, confici

curavi. Ceterum quum instrumenta optica nullibi terrarum meliora quam Londini conficiantur, te etiam atque etiam oro, ut optima eorum, quae sequens designatio exhibet, pro me eligas, & Domino Claesson (cui curam numorum pro iis solvendorum, & transmittendorum Holmiam instrumentorum comisi) tradi facias. Certissimus ero me bona habiturum instrumenta, si tu, harum rerum intelligentissimus Judex ea elegeris & approbaveris. Si aliqua fuerint, quae apud artifices statim haberi non poterunt, ea mihi primum transmittas quae haberi non poterunt, ea mihi primum transmittas quae haberi possunt, reliqua etiam missurus, quam primum parata fuerint. Optarim enim, ut ante hyemem, quam potero plurima habeam. Si aliqua ratione heic locorum utilis tibi esse potero senties gratam animi voluntatem mihi nunquam defuturam.

#### Designatio Instrum.

---

Vitra ad Tubum Astron. 16 pedd. circiter.

Vitra ad Tub. Astron. 8 ped.

Prismata et Lentes ad Newt. Theoriam Colorum demonstrandam.

Laterna Magica cum figuris necessariis.

Lens pro Camera obscura 4 ped.

Specula Conica & Cylindrica cum picturis deformibus.

Plana vitrea inter quae aqua ascendit in figura hyperbolica.

Oculus artificialis.

Tubus vitreus amplus pro electricitate vitri monstranda.

Microscopium duplex cum apparatu necessario.

Instrumenta pro Legibus Refractionis & Reflexionis detegendis.

Duo vitra concava pro Myopibus foc. unius pedis.

Diaboli Cartesiani.

Praeterea etiam libros nonnullos novos apud vos noviter editos libenter desideraverim, ut D<sup>ni</sup> Smith Systeme of Opticks: D<sup>ni</sup> MacLaurin Systema Algebrae, & si qui alii recens editi fuerint in Mathematicis, novi quid continentes, quales credo in Anglia, ingeniorum feracissima non deesse. ante alios aveo scire, utrum D<sup>ni</sup> Machin Theoria Gravitationis lucem viderit, vel quando videbit & quomodo valeant insignes viri

fautoresque mei honoratissimi D<sup>ni</sup> Halleyus, Moivreus, Machin, quibus meis verbis salutem plurimam impertias. Vale interim & fave

Tui Studiosissimo

Holmiae d 19

S. KLINGENSTIERNA

Septembris 1738.

Problems of Klingenstierna (1733 ?)

*Problema* Sint in  $A$  &  $a$  duo ignes, quorum vires calefaciendi in distantiiis aequalibus sint in data ratione  $AF$  ad  $af$ , & crescentibus distantiiis decrescant in ratione quadratorum distantiarum. Quaeritur per quam viam ab ignibus illis recedere debeat viator in loco aliquo dato  $S$  constitutus, ut minimum sentiat calorem.

*Solutio* Sit  $BD$  particula quam minima viae, qua viator a puncto quocunque  $B$  recedere debet, ut ab ignibus  $A$  et  $a$  minimum calorem sentiat. Centro  $B$  intervallo  $BD$  describatur circumferentia circuli  $DK$ , & erit intensitas caloris in  $D$  minor intensitate, ejus in quovis alio circumferentiae  $DK$  puncto. Quare si in circumferentia illa sumatur punctum  $d$  puncto  $D$  proximum, calor in  $d$  per naturam minimi aequalis censi potest calori in  $D$ . Sed calor in  $D$  per hypoth. est  $\frac{AF}{AD^2} + \frac{af}{aD^2}$  & calor in  $d$ ,  $\frac{AF}{Ad^2} + \frac{af}{ad^2}$ , Ergo  $\frac{AF}{AD^2} + \frac{af}{aD^2} = \frac{AF}{Ad^2} + \frac{af}{ad^2}$ , & transponendo  $\frac{AF}{AD^2} - \frac{AF}{Ad^2} = \frac{af}{ad^2} - \frac{af}{aD^2}$ .

Centris  $A$  &  $a$  intervallis  $AD$  &  $ad$  describantur arcus  $Dp$  &  $dP$ , rectis  $Ad$  &  $aD$  occurrentes in  $p$  &  $P$ , & per principia methodi infinitesimalis erit  $\frac{1}{AD^2} - \frac{1}{Ad^2} = \frac{2dp}{AD^3}$  &  $\frac{1}{ad^2} - \frac{1}{aD^2} = \frac{2DP}{aD^3}$ , adeoque aequatio  $\frac{AF}{AD^2} - \frac{AF}{Ad^2} = \frac{af}{ad^2} - \frac{af}{aD^2}$  mutatur in hanc,  $\frac{2dp \cdot AF}{AD^3} = \frac{2DP \cdot af}{aD^3}$ , & dividendo per 2, ac pro  $AD$   $ad$ , scribendo  $AB$   $aB$ ,  $\frac{dp \cdot AF}{AB^3} = \frac{DP \cdot af}{aB^3}$ .

Centris  $A$  &  $a$  intervallis  $AB$ ,  $aB$  describantur arcus  $BE$  &  $Be$  rectis  $AD$  &  $aD$  occurrentes in  $E$  &  $e$ , & erit triangulum  $DBE$  simile triang.  $Ddp$ , triangulum  $DBa$  simile triang.  $dDP$ . Quare  $DB:Dd = BE:dp$ , &  $DB:Dd = Be:DP$ , adeo-

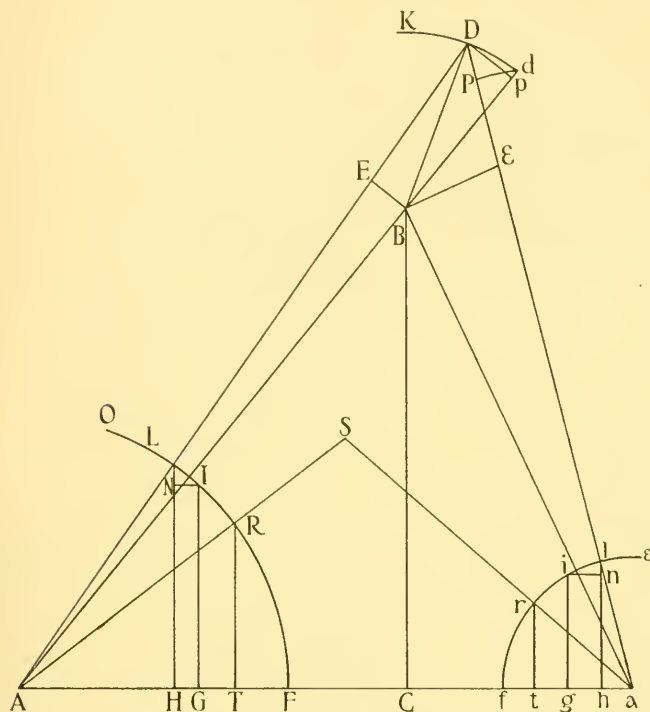


FIG. 33.

que ex aequo  $BE:dp = Be:DP$  Si itaque in aequatione  $\frac{dp \cdot AF}{AB^3} = \frac{DP \cdot af}{aB^3}$  pro  $dp$  &  $DP$  substituantur earum proportionales  $BE$  &  $Be$ , habetur  $\frac{BE \cdot AF}{AB^3} = \frac{Be \cdot af}{aB^3}$ .

Centris  $A$  &  $a$  intervallis  $AF$  &  $af$  describantur circumferentiae  $FQ$  &  $fq$ , rectis  $AB$ ,  $AD$ , atque  $aB$ ,  $aD$  occurrentes in  $I$ ,  $L$ , &  $i$ ,  $l$ , eritque ob similitudinem triangulorum  $ABE$ ,  $AIL$ ,  $AB:BE = AI$  (id est  $AF$ ):  $IL$ , unde  $\frac{BE \cdot AF}{AB^3} = \frac{IL}{AB^3}$ .

Similiter ob similitudinem triangulorum  $aBe, ail$ , erit

$$aB : Be = ai \text{ (id est } af) : il, \text{ unde } \frac{Be \cdot af}{aB} = il.$$

Ergo si in aequatione  $\frac{BE \cdot AF}{AB^3} = \frac{Be \cdot af}{aB^3}$  pro  $\frac{BE \cdot AF}{AB}$  &  $\frac{Be \cdot af}{aB}$  substituantur  $IL$  &  $il$ , transit illa in hanc:  $\frac{IL}{AB^2} = \frac{il}{aB^2}$ .

Ad rectam  $Aa$  demittantur normales  $LH, IG, BC, ig, lh$ , ipsique  $Aa$  parallelae  $IN, in$ , rectis  $LH, lh$  occurrentes in  $N, n$ . Propter similitudinem triangulorum  $ABC, AIG, LIN$ , est

$$AB : BC = AI \text{ (id est } AF) : IG,$$

$$\& \quad AB : BC = \quad \quad \quad LI : IN;$$

quare terminis ordinatim in se ductis

$$AB^2 : BC^2 = AF \times LI : IG \times IN, \text{ unde } \frac{IL}{AB^2} = \frac{IG \cdot IN}{AF \cdot BC^2}.$$

Similiter propter similitudinem triangulorum  $aBC, aig, lin$ , est

$$aB : BC = ai \text{ (id est } af) : ig$$

$$aB : BC = \quad \quad \quad li : in;$$

quare terminis ordinatim in se ductis

$$aB^2 : BC^2 = \quad \quad \quad af \cdot li : ig \cdot in; \text{ unde } \frac{il}{aB^2} = \frac{ig \cdot in}{af \cdot BC^2}$$

$$\text{Sed inventum erat } \frac{IL}{AB^2} = \frac{il}{aB^2}, \text{ ergo } \frac{IG \cdot IN}{AF \cdot BC^2} = \frac{ig \cdot in}{af \cdot BC^2},$$

& multiplicando per  $BC^2$ ,  $\frac{IG \cdot IN}{AF} = \frac{ig \cdot in}{af}$ . Est vero  $IG \cdot IN$  elementum circuli  $IGHL$ , &  $ig \cdot in$  elementum circuli  $ighl$  quare  $\frac{IGHL}{AF} = \frac{ighl}{af}$ , adeoque  $\frac{AF}{af} = \frac{IGHL}{ighl}$ .

Sit  $S$  locus datus unde prodit viator. Jungantur  $AS, aS$  circumferentiis  $FQ, fq$  occurrentes in  $R, r$  & demittantur  $RT, rt$  perpendiculares ad  $Aa$ . Et cum per jam demonstrata, elementa  $IGHL, ighl$  ubique sint in data ratione  $AF$  ad  $af$ , erit etiam componendo, Summa  $IGHL$ , id est spatium  $RTHL$ , ad sumam omnium  $ighl$ , id est spatium  $rthl$ , in eadem data ratione  $AF$  ad  $af$ , unde sequens prodit *Constructio*.

Centris  $A$  &  $a$  descriptis circulis  $FQ, fq$ , quorum radii  $AF, af$  sint proportionales viribus calefaciendi ignium  $A$  &  $a$ , jungantur  $AS$  &  $aS$ , circulis illis occurrentes in  $R$  &  $r$ , & demittantur

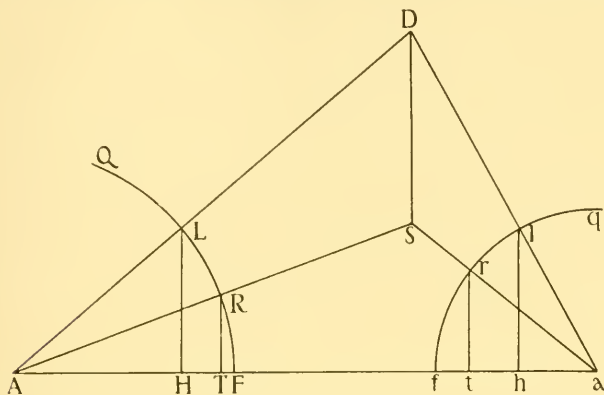


FIG. 34.

*Rt*, *rt*, normales ad *Aa* Rectis *LH*, *lh*, itidem normalibus ad *Aa*, abscindantur Spatia *TRLH*, *trlh*, quae sint in ratione *AF* ad *af*. Jungantur & producantur *AL* & *al*, donec conveniant in *D*, & erit punctum *D* in curva quaesita *SD*.

*Problema.* Invenire curvas  $AGBC$  &  $AHBI$ , quarum talis est ad se invicem relatio, ut curva prior  $AGBC$  rotata circa polum fixum  $A$  semper secetur ab altera  $AHBI$  in punctis summis  $B$ ,  $b$ , & ut segmenta  $AGBA$ ,  $AHBA$  semper sint in data ratione  $m$  ad  $n$ .

*Solutio.* Rotetur curva  $AGBC$  circa punctum fixum  $A$ , donec perveniat in situm proximum  $AFDC$ , in quo situ secetur a curva  $AHBI$  in  $b$ . Centro  $A$  intervallo  $AB$  describatur arcus  $BD$  curvae occurrens in  $D$ , & jungantur  $AD$ ,  $Ab$ , quarum haec occurrat arcui  $BD$  in  $E$ . Et quia  $AGBA : AHBA = m : n$ , &  $AFbA : AHbA = m : n$ , erit etiam dividendo

$$AFbA - AGBA : AHbA - AHBA = m : n,$$

id est, Triangulum  $ADb$ :triang.  $ABb = m:n$ , unde ob basin communem  $Bb$ , erit  $DE:BE = m:n$ .

Dicatur  $AD, x$ ;  $Eb, dx$ ;  $DE, dy$ ; & erit  $EB = \frac{n DE}{m}$ ,  
&  $BD = DE + \frac{n DE}{m} = \frac{m+n}{m} dy$ .

Et quoniam per hyp. tangens curvae  $IGBC$  in  $B$  parallela est tangenti ejusdem in  $b$ , erit angulus rotationis  $BAD$  aequalis angulo quem duae rectae ad curvam normales in punctis  $D$  &  $b$  constituunt in centro circuli osculatoris. Ergo  $AD:DB =$  radius curvedinis in  $D$ : ad elementum curvae  $Db$ , id est

$$(\text{dicto } Db = ds)x : \frac{m+n}{m} dy = \frac{x ds^2 dx}{dx dy ds - x dy dds} : ds,$$

$$\text{adeoque } x ds = \frac{\frac{m+n}{m} dy \cdot x ds^2 dx}{dx dy ds - x dy dds} \text{ vel } 1 = \frac{\frac{m+n}{m} ds dx}{ds dx - x dds},$$

$$\text{unde} \quad ds dx - x dds = \frac{m+n}{m} ds dx,$$

$$\text{seu} \quad -x dds = \frac{n}{m} ds dx, \text{ hinc } \frac{n}{m} \frac{dx}{x} = -\frac{dds}{ds},$$

sumtisque logarithmis  $\frac{n}{m} l \frac{x}{a} = l \frac{dy}{ds}$ , & perficiendo quod restat reductionis:

$$\frac{x^{\frac{n}{m}} dx}{\sqrt{a^{\frac{2n}{m}} - x^{\frac{2n}{m}}}} = dy$$

Centro  $A$ , intervallo a describatur circulus, cujus elementum rectis  $AD$ ,  $Ab$  comprehensum dicatur  $dz$ , eritque  $x:dy = a:dz$ , unde  $dy = \frac{x dz}{a}$ , & hoc valore substituto in aequatione modo inventa

$$\frac{x^{\frac{n}{m}} dx}{\sqrt{a^{\frac{2n}{m}} - x^{\frac{2n}{m}}}} = dy,$$

transformatur illa in hanc

$$\frac{x^{\frac{n}{m}} dx}{\sqrt{a^{\frac{2n}{m}} - x^{\frac{2n}{m}}}} = \frac{x dz}{a},$$

seu

$$\frac{a x^{\frac{n}{m}-1} dx}{\sqrt{a^{\frac{2n}{m}} - x^{\frac{2n}{m}}}} = dz.$$

Ponatur  $x = a \frac{v}{a} \Big| \frac{m}{n}$ , & aequatio transibit in hanc;

$$\frac{m}{n} \frac{adv}{\sqrt{aa-vv}} = dz$$

quæ sequentem suppeditat Problematis Constructionem. Centro  $A$  intervallo quovis  $AB$  describatur circumferentia circuli, in qua hinc inde a puncto quovis dato  $B$  sumantur arcus  $BC$ ,  $BD$

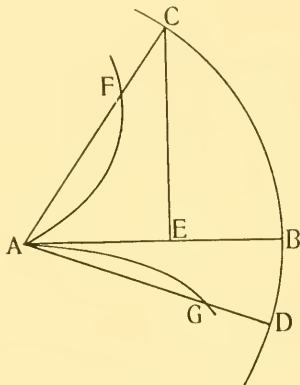


FIG. 35.

in ratione  $n$  ad  $m$ . Jungantur  $AC$ ,  $AD$  & a puncto  $C$  demittatur  $CE$  normalis ad radium  $AB$ . In  $AC$  &  $AD$  sumantur  $AF$  &  $AG$  aequales  $AB \cdot \frac{CE}{AB} \Big| \frac{m}{n}$  & erit punctum  $F$  in curva fixa  $AHBT$ , & punctum  $G$  in curva rotatili  $AGBC$ .

Coroll. Si fuerit  $m$  ad  $n$  ut numerus ad numerum, utraque curvarum est Algebraica, sive minus, earum constructio dependet a multisectione anguli & rationis, seu quod idem est quadratura circuli & hyperbolæ.

# IX

## MACHIN AND STIRLING

(1)

*Machin to Stirling (1733?)*

To

Mr Stirling at the Academy  
in little Tower Street

Dear Sir

I intend to give you some short notes upon Mr Bernoulli's Letter, w<sup>ch</sup> if you approve of it shall be addrest in a Letter to yourself. It shall be ready against the beginning of next week, unless anything material happen to hinder it. I have reason to believe that if he be a man of any candour, I shall be able to give him entire satisfaction as to every objection that he makes, & do intend withal to oblige him w<sup>th</sup> the solution of a Problem w<sup>ch</sup> I now percieve he had proposed to himself but quitted rather than be at the pains to go through w<sup>th</sup> it. And that is whether there be a point in his locus from whence the Planet will appear to move equally swift in the Apsides & one of the middle distances. And where it is that y<sup>e</sup> point lyes. As I apprehend he may have communicated some of his remarks to others as well as yourself or may have hinted that he has made some; I should be glad to

a word or line

know by the bearer, whether you will give me leave to shew this Letter to the Society upon the foot of there being some new Problems in it, w<sup>ch</sup> may furnish me w<sup>th</sup> the opportunity of saying that his Objections are to be answered. I do not mean to have the Letter read, but only to have the Contents of it mentioned & especially the Problems since he seems to have sent those on purpose to be proposed to others. I shall

herein behave according to the directions you are pleased to give.

Er. Your most faithful  
Friend & very humble Serv<sup>t</sup>

Thursday morning

J. MACHIN.

(2)

*Machin to Stirling, 1738*

Dear Sir

Gresham College June 22. 1738

The date of your obliging Letter when I cast my eye upon it gives me great concern. I was ashamed when I received a Letter from you to think you had prevented me in paying my respects to you first, but am now confounded in the reflection of having slipt so long a time without returning an answer to it. Sure I am in the case of Endymion! But every day has brought its business and its impertinence to engage me and to interrupt me. Were there time I could plead perhaps more things in my excuse than you may be apt to imagine. This long vacation which begins today, appears, if it deceive me not in my expectation, as one of y<sup>e</sup> greatest blessings I have long since enjoyed. If I am tardy after this, then believe (what would grieve me if you should believe) that you are one that are not in my thoughts. Think not that you are singular in your retirement from y<sup>e</sup> world. There may I can assure you be as great a solitude from acquaintance & conversation in a Town as in a Desert. But of this sufficient.

Mons<sup>r</sup> Maupertuis has sent you a present of his book which I have deliverd to Mr Watts for you. It contains a complete account of the measurement in the North. Mr Celsius likewise published two or 3 sheets on y<sup>e</sup> same subject chiefly to shew that Cassini's measurement was far inferior to this in point of exactness, and which I suppose you will need no argument to prove when you have read over M. Maupertuis's book.

We have also had from time to time scraps of accounts communicated to us, still in expectation of something more perfect, w<sup>ch</sup> I intended to have sent to you, but this book has rendered it unnecessary.

There have been great wrangles and disputes in France about this measurement. Cassini has endeavoured to bring the exactness of it into Question. Because the Gentlemen did not verify the truth of their astronomical observations, by double observation with y<sup>e</sup> face of their Instrument turned contraryways. So that M<sup>r</sup> Maupertuis was put to the necessity of procuring from England a certificate concerning the construction of M<sup>r</sup> Graham's Instrument, to show that it did not need that sort of verification.

You will see that this measurement in y<sup>e</sup> North, if it be compared with y<sup>t</sup> in France, will serve to prove that y<sup>e</sup> figure is much more oblate than according to y<sup>e</sup> rule. But perhaps it will be safer to wait for the account from Peru before any conclusion be drawn. These Gentlemen have also compleated their work and are returning home where they are expected in a short time.

Mons<sup>r</sup> De Lisle has published a Memoir read in the Academy at Petersburg w<sup>ch</sup> contains y<sup>e</sup> scheme of a Grand Project of the Czarina for making a compleat Mapp of her whole Empire, and in w<sup>ch</sup> there is a design of making such a measurement not only from North to South but from East to West also as will far surpass any thing that was ever yet thought of; it being to contain above 20 degrees of y<sup>e</sup> meridian and many times more in the parallels.

Your Proposition concerning y<sup>e</sup> figure (wherein all my friends can witness how much I envy you) could never find a time to appear in the world with a better grace than at present, Now when y<sup>e</sup> great Princes of y<sup>e</sup> Earth seem to have their minds so fix't upon it.

But for other reasons I should be glad if your Proposition could be published in some manner or other as soon as possible, but not without some investigation at least: unless you have hit upon a Demonstration w<sup>ch</sup> would be better, because I find several people are concerning themselves upon that subject. I have kept your paper safe in my own custody, nor has any one had the perusal of it.

Nor shall I believe that any one will find it out till I see it. But M<sup>r</sup> Macklaurin in a Letter to me dated in february last, (and w<sup>ch</sup> was not deliver'd to me but about a month ago, the Gentleman being ill to whose care it was entrusted) taking occasion to speak of y<sup>e</sup> figure of y<sup>e</sup> Earth, and that

S<sup>r</sup> Is. had supposed but not demonstrated it to be a Spheroid, proceeds on in the following words, ' Mr Stirling if I remember right told me in April that none of those who have considered this subject have shewed that it is accurately of that figure. I hit upon a demonstration of this since he spoke to me w<sup>ch</sup> seems to be pretty simple.'

I have given you his own words for fear of a mistake, because I am surprised you did not take that opportunity to inform him, that you had found it to be of that figure. For that nobody has yet shewn it to be so is what I thought everybody had known. But I shall take this opportunity to advise him to communicate his demonstration to you.

And if he has found out a simple demonstration for it, I think it ought to be highly valued, for it does not seem easy to come at it. I own I have not had time to pursue a thought I had upon it, and which I apprehended and do still apprehend might lead to the demonstration and shall be very glad if he or any one else by doing it before shall save me that trouble.

As to y<sup>e</sup> Invention of Mr Euler's Series were I in your case I would not trouble myself about it, but let it take its own course, if anything should arise your Letter to me w<sup>ch</sup> I shall keep will be a sufficient acquittal of yourself.

Mr Moivre's Book is now published but I have not got it yet nor have I been able to see him but once since I rec<sup>d</sup> your Letter and as to this conveyance I was but just now apprized of it and have but just time to get this ready before Mr Watts goes out of Town.

As to y<sup>e</sup> moon's Distance I have now materials to fix y<sup>e</sup> moon's Parallax, and chiefly by means of an Observation of the last Solar Eclipse at Edinburgh by Mr Macklaurin, and will take care as soon as I can make y<sup>e</sup> calculation to send it to you.

There are some other matters whereto I should speak which I must now defer to another opportunity, and only say now that I am with affectionate regard

Your most faithful friend

& very humble Servant

JOHN MACHIN.

## X

### CLAIRAUT AND STIRLING

(1)

*Clairaut to Stirling, 1738*

Monsieur

En cas qu'un Memoire sur la Figure de la Terre que j'envoyai de la Laponie à la Société Royale, soit parvenu jusqu'à vous et que vous l'ayés daigné lire, vous y aurés reconnû plusieurs Theorêmes dont vous aviés donné auparavant les enoncés, parmi les belles decouvertes dont est rempli un morceau que vous avés inseré dans les *transact. Philosoph.* de l'année 1735 ou 1736. Vous aurés été peut-être etonné que traitant la même matiere que vous je ne vous aye point cité. Mais je vous supplie d'être persuadé que cela vient de ce que je ne connoissois point alors votre Memoire, et que si je l'eusse lû je me serois fait autant d'honneur de le citer que j'ai ressenti de plaisir lorsque j'ai appris que je m'étois rencontré avec vous.

Depuis le tems où j'ai donné cette Piece j'ai poussé mes recherches plus loin sur la même matiere, et j'envoye actuellement mes nouvelles decouvertes à la Société Royale. Après vous avoir fait ce recit Monsieur et vous avoir prié d'excuser la liberté que j'ai pris de vous ecrire sans avoir l'honneur d'être connu de vous, oserois je vous demander une grace, c'est de vouloir bien jeter les yeux sur mon second Memoire que M<sup>r</sup> Mortimer vous remettra si vous le daignés lire.

Ce n'est pas seulement l'envie d'être connu de vous qui m'engage a vous prier de me faire cette grace, Mais c'est que j'ai appris par un ami qui a vu a Paris un Geometre anglois appellé M. Robbens que vous aviés depuis peu travaillé sur la même matiere.

Je souhaiterois donc extremement de seavoir si j'ai été asses heureux encore pour m'être rencontré avec vous. Si au contraire je m'étois trompé je vous serois infiniment obligé de me le dire franchement afin que je m'en corrigiasse. Quoi qu'il en soit si vous daignés me donner quelques momens, vous aurés bientôt vû de quoy il est question et si mon memoire m'attire une reponse de vous je serai charmé de l'avoir fait parce qu'il y a deja longtems que je souhaite d'être en liaison avec vous. Quelqu'envie que j'en aye ne croyés pourtant pas Monsieur que je soye asses indiscret pour vous importuner souvent par des lettres inutiles pleines de simples complimens. Mr Mortimer pourra vous dire quelle est ma conduite a son egard, J'en oserai de même avec vous si vous me le permettés. En attendant j'ai l'honneur d'être avec estime et respect

Monsieur

Votre très humble et très

à Paris le 2 Octobre 1738

obeissant Serviteur

CLAIRAUT.

P.S. En cas que vous veuillés me faire reponse il faudra avoir la bonté de remettre votre lettre a M. Mortimer. Si vous n'aimés a ecrire en françois, je dechiffre assés d'anglois pour entendre une lettre et quand ma science en cete langue ne suffiroit pas, j'aurois facilement du secours.

# XI

## EULER AND STIRLING

(1)

*Stirling to Euler, 1738*<sup>1</sup>

Celeberrimo Doctissimoque Viro

Leonhardo Euler

S.P.D

Jacobus Stirling

Tantum temporis elapsum est ex quo dignatus es <sup>mihi</sup> (ad me) scribere, ut jam rescribere vix ausim nisi tua humanitate fretus. Per hosce duos annos plurimis negotiis implicitus sum, quae occasionem mihi dederunt frequenter eundi in Scotiam et dein Londinum redeundi. Et haec in causa fuerunt tum quod epistola tua sero ad manus meas pervenit, tum quod in hunc usque diem vix suppeterat tempus eundem perlegendi ea qua meretur attentione. Nam postquam speculationes sunt diu interruptae, ne dicam obsoletae, patientia opus est antequam induci possit animus iterum de iisdem cogitare. Hanc igitur primam corripio occasionem testandi meam in te Observantiam et simul (gratias) agendi gratias dudum debitas propter literas eximiis inventis refertas.

Gratissimum mihi fuit Theorema tuum pro summandis Seriebus per aream Curvae et differentias sive fluxiones Terminorum quippe generale et praxi expeditum. <sup>Statim</sup> (Illius) percepi item extendi ad plurima serierum genera, et quod <sup>celerime</sup> praecipuum et <sup>^</sup> plerumque (celeriter) approximatur. Forte non observasti theorema meum pro summandis Logarithmis <sup>Tui</sup> nihil aliud esse quam casum particularem tui Theorematis <sup>^</sup>

<sup>1</sup> This is only Stirling's rough draft with all his corrections. Erasures are indicated by brackets.

generalis; (quod ingenue fateor). Sed et <sup>eo</sup>  $\wedge$  gratius mihi fuit  
 (tuum) hunc inventum, (quoniam) <sup>quod</sup> de eodem (ego) quoque ego  
 olim cogitaveram; sed ultra primum terminum non processi,  
 et per eum solum <sup>approximav</sup> <sup>pro libitu</sup> (perveni satis expedite) ad valores Serierum  
 satis expedite

$\wedge$  scilicet per repetitionem calculi, ut in resolutione aequa-  
 tionum affectarum; cujusque specimen dedi (plurimis abhinc  
 annis) in philosophicis nostris transactionibus:

Quae habes de inveniendis Logarithmis per Seriem Harmo-  
 nicam (non percipio, propter novi) obscura mihi <sup>saltem</sup>  $\wedge$  videntur,  
 quoniam  $\wedge$  non recte intelligo (notationem.)

Imprimis autem mihi placuit methodus tua summandi  
 quasdam Series per potestates peripheriae circuli, (quarum  
 indices sunt numeri pares). Hoc fateor (omnino novum et)  
 admodum ingeniosum <sup>et omnino novum</sup>  $\wedge$  nec video quod <sup>habeat</sup>  $\wedge$  quicquid  
 (affin <sup>commune</sup> habeat) cum (iis quae hactenus publicantur,) <sup>methodis receptis</sup> adeo ut  
 facile <sup>credam</sup> (concedam) te idem <sup>hausisse</sup> (hausisse) ex novo fonte  $\wedge$  ((et  
 nullus dubito te hactenus observasse, aut certe ex fundamento  
 tuo facile percipies, alias series tuis tamen affines summari  
 posse per potestates peripheriae quarum indices sunt numeri  
 impares. Verbi gratia, denotante  $p$  periferia,

$$\frac{1}{4}p = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \&c \text{ ut vulgo notum}$$

$$\frac{1}{32}p^3 = 1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} + \frac{1}{9^3} - \&c$$

$$\frac{5}{1536} = 1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} + \frac{1}{9^5} - \&c$$

(&c.)

Series tuae <sup>continentur in</sup> (comprehenduntur sub) forma generali

$$1 + \frac{1}{2^n} + \frac{1}{3^n} + \frac{1}{4^n} + \frac{1}{5^n} + \frac{1}{6^n} + \&c$$

ubi  $n$  est numerus par) eadem (tamen ad formulam sequen-

tem) nullo negotio reducitur, (scilicet) reducitur ad formulam sequentem,

$$1 + \frac{1}{3^n} + \frac{1}{5^n} + \frac{1}{7^n} + \frac{1}{9^n} + \frac{1}{11^n} + \&c$$

(ubi termini alterni desunt, et omnes sub hac forma comprehenduntur et hanc summam  
hensas summam)  $\wedge$  doces  $\wedge$  per potestatem periferiae  
ejus index est  $n$  modo sit Ceterum si

(quando  $n$  est) numerus par. (Si jam mutantur) signa  
terminorum alternorum mutantur ut evadat Series

$$1 - \frac{1}{3^n} + \frac{1}{5^n} - \frac{1}{7^n} + \frac{1}{9^n} - \frac{1}{11^n} + \&c.$$

Haec inquam semper summari potest (atque haec Series,  
quando  $n$  est numerus impar summari potest) per dignitatem  
periferi (circuli) ejus index est  $n$ . <sup>modo sit numerus impar</sup> (verbi gratia) utique si sit  
 $n = 1$ , (erit)  $\frac{1}{4}\rho = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \&c$  ut vulgo notum

$$n = 3, \frac{1}{32}\rho^3 = 1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} + \frac{1}{9^3} - \frac{1}{11^3} + \&c$$

$$n = 5, \frac{5}{1536}\rho^5 = 1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} + \frac{1}{9^5} - \frac{1}{11^5} + \&c$$

Et nullus dubito te haecenus idem observasse, aut saltem  
facile observatur ex fundamento tuo quod libenter videbo,  
quando (animus erit tibi idem impertire) ita tibi visum fuerit.

Hic autem <sup>monendus es</sup> (aequum est ut te moneam) D. Maclaurin <sup>Matheseos</sup>  $\wedge$  pro-  
fessorem (Matheseos) Edinburgi, post aliquot tempus (brevis)  
<sup>jam</sup>  
editurum librum de fluxionibus ejus paginas aliquot (haecenus)  
impressas (mecum) mecum communicavit in quibus duo habet  
Theoremata pro summandis seriebus per differentias termi-  
norum, quorum alterum ipsissimum est quod tu dudum  
mihi

(ad me) misisti, (et ejus ego eum illico certiore feci). Et  
etiam si ille libenter promiserat se idem testaturum in sua  
praeefatione, judicio tamen tuo submitto annon velles (edere)

tuam epistolam  $\wedge$  in nostris philosophicis transactionibus.

Et si vis quaedam illustrare vel demonstrare, (aut plura  
 adjicere, ego aut) et cito mihi rescribere, curabo (tuam epistolam  
 viseram lucem diu) antequam ejus liber prodierit. Quod si  
 animus erit hac (data) occasione eligi unus ex Sociis nostrae  
 Societatis  
 (Academiae) Regiae, idem reliquis gratum (esse non) procul  
 dubio gratum erit (postquam inventa tua viderint Et) mihi  
 vero semper gratissimum ut amicitiam (mihi licet immerenti)  
 continuare digneris

Edinburgi 16 Aprilis 1738

(2)

*Euler to Stirling, 1738*

Illustrissimo atque Celeberrimo Viro

Jacobo Stirling

S. P. D.

Leonhard Euler

Quo majore desiderio litteras a Te Vir Celeb. expectavi, eo  
 majore gaudio me responsio Tua humanissima affecit, qua,  
 eo magis sum delectatus, quod non solum litteras meas Tibi  
 non ingratis fuisse video, sed Temet etiam ad commercium  
 hoc inceptum continuandum invitare. Gratias igitur Tibi  
 habeo maximas, quod tenues meas meditationes tam benevole  
 accipere Tuumque de iis judicium mecum communicare  
 volueris. Epistolam autem meam a Te dignam censi, quae  
 Transactionibus Vestris inseratur, id summae Tuae tribuo  
 humanitati, atque in hunc finem nonnullas amplificationes et  
 dilucidationes superaddere visum est, quas pro arbitrio vel  
 adjungere vel omittere poteris. Hac autem in re quicquam  
 laudis Celeb. D. Maclaurin derogari minime velle, cum is  
 forte ante me in idem Theorema seriebus summandis oscerviens  
 incidit, et ideo primus ejus Inventor nominari mereatur.  
 Ego enim circiter ante quadriennium istud Theorema inveni,  
 quo tempore etiam ejus demonstrationem et usum coram  
 Academia nostra fusius exposui, quae dissertatio mea pariter  
 ac illa, quam de Summatione Serierum per potestates peri-

pherie circuli composui in nostris Commentariis, qui quotannis prodeunt, brevi lucem publicam aspiciet. In Commentariis autem nostris jam editis aliquot extant aliae methodi meae Series summandi quarum quaedam multum habent Similitudinis cum Tuis in egregio Tuo opere traditis, sed quia tum temporis Tuum methodum differentialem nondum videram, ejus quoque mentionem facere non potui, uti debuissim. Misi etiam jam ante complures annos ad Illustris. Praesidem Vestrum D. Sloane schediasma quodpiam, in quo generalem constructionem hujus aequationis

$$\dot{y} = yy\dot{x} + ax^m\dot{x}$$

dedi, quae aequatio ante multum erat agitata, at paucissimis tantum casibus exponentis  $m$  constructa. Hae igitur Dissertatio, si etiamnum praesto esset, simul tanquam specimen produci posset, coram Societate vestra, quando me pro membro recipere esset dignatura, quem quidem honorem Tibi Uni Vir Celeber. deberem. Sed vereor ut Inelytae Societati expediat me Socium eligere, qui ad Academiam nostram tam arete sum alligatus, ut meditationes meas qualescunque hic primum producere teneam.

Ut autem ad Theorema, quo summa cujusque Seriei ex ejus termino dicto generali inveniri potest, revertar, perspicuum est formulam datam eo majorem esse allaturam utilitatem, quo ejus plures termini habeantur, summa autem difficile esse videtur, eam quousque lubuerit, continuare. Equidem ad plures quam duodecim terminos non pertetigi, quorum ultimos non ita pridem demum inveni; haec autem expressio se habet ut sequitur.

Si Seriei cujuscunque terminus primus fuerit  $A$ , secundus  $B$ , tertius  $C$ , etc. isque ejus index est  $x$  sit  $= X$ : erit summa hujus progressionis, puta

$$\begin{aligned} A + B + C + \text{etc} \dots + X &= \int X dx + \frac{X}{1.2} + \frac{dX}{1.2.3.2dx} \\ &\quad - \frac{d^3 X}{1.2.3.4.5.6dx^3} + \frac{d^5 X}{1.2.3.4.5.6.7.6dx^5} \\ &\quad - \frac{3d^7 X}{1.2.3\dots 9.10dx^7} + \frac{5d^9 X}{1.2.3\dots 11.6dx^9} \end{aligned}$$

$$\begin{aligned}
& - \frac{691d^{11}X}{1.2.3 \dots 13.210dx^{11}} + \frac{35d^{13}X}{1.2.3 \dots 15.2dx^{13}} \\
& - \frac{3617d^{15}X}{1.2.3 \dots 17.30dx^{15}} + \frac{43867d^{17}X}{1.2.3 \dots 19.42dx^{17}} \\
& - \frac{1222277d^{19}X}{1.2.3 \dots 21.110dx^{19}} \text{ etc.}
\end{aligned}$$

ubi fluxio  $dx$  constans est posita.

Haec autem expressio parumper mutata etiam ad summam seriei a termino  $X$  in infinitum usque inveniendam accommodari potest. Hujus vero formae praeter insignem facilitatem, quam suppeditat ad summas proxime inveniendas, eximius est usus in veris summis serierum algebraicarum investigandis, quarum quidem summae absolute exhiberi possunt, ut si quaeratur summa hujus progressionis potestatum

$$1 + 2^{12} + 3^{12} + 4^{12} + 5^{12} + \dots + x^{12},$$

erit  $X = x^{12}, \quad \int X dx = \frac{1}{13} x^{13}, \quad \frac{dX}{dx} = 12x^{11},$

$$\frac{d^3 X}{dx^3} = 10.11.12.x^9, \text{ et ita porro, donec } \frac{d^{13} X}{dx^{13}}$$

una cum sequentibus Terminis = 0

Hinc igitur resultabit summa quaesita =

$$\frac{x^{13}}{13} + \frac{x^{12}}{2} + x^{11} - \frac{11x^9}{6} + \frac{22x^7}{7} - \frac{33x^5}{10} + \frac{5x^3}{3} - \frac{691x}{2730},$$

quam summam nescio, an ea per ullam aliam methodum tam expeditam inveniri queat. Potest autem hac ratione aeque commode definiri summa hujus progressionis

$$1 + 2^{21} + 3^{21} + 4^{21} + \dots + x^{21},$$

quod per alias vias labor insuperabilis videtur.

Sin autem seriei propositae termini alternativivi signis + et - fuerint affecti, tum theorema istud minus commode adhiberi posset, quia ante binos terminos in unum colligi oporteret. Pro hoc igitur serierum genere aliud investigavi Theorema priori quidem fere simile, quod ita se habet.

Si quaeratur summa hujus seriei

$$A - B + C - D + \dots \pm X,$$

ubi  $X$  sit terminus cujus exponens seu index est  $x$ , habetque signum vel  $+$  vel  $-$  prout  $x$  numerus erit vel impar vel par. Dico autem hujus progressionis summam esse

$$\begin{aligned}
 = \text{Const. } \pm & \left( \frac{X}{12} + \frac{dX}{1.2.2dx} - \frac{d^3X}{1.2.3.4.2dx^3} \right. \\
 & + \frac{3d^5X}{1.2.3 \dots 6.2dx^5} - \frac{17d^7X}{1.2.3 \dots 8.2dx^7} \\
 & + \frac{155d^9X}{1.2.3 \dots 10.2dx^9} - \frac{2073d^{11}X}{1.2.3 \dots 12.2dx^{11}} \\
 & \left. + \frac{33227d^{13}X}{1.2.3 \dots 14.2dx^{13}} - \&c. \right)
 \end{aligned}$$

Constantem autem ex uno casu, quo summa est cognita, determinari oportet.

At si series summanda connexa sit cum Geometrica progressionem hoc modo

$$An + Bn^2 + Cn^3 + \dots + Xn^x$$

tum minus congrue utrumque praeccedentium theorematum adhiberetur. Summa enim commodius invenietur ex hac expressione

$$\begin{aligned}
 \text{Const. } + n^x & \left( \frac{nX}{n-1} - \frac{\alpha dX}{1(n-1)^2 dx} + \frac{\beta d^2X}{1.2(n-1)^3 dx^2} \right. \\
 & - \frac{\gamma d^3X}{1.2.3.(n-1)^4 dx^3} + \frac{\delta d^4X}{1.2.3.4(n-1)^5 dx^4} - \text{etc.} \left. \right)
 \end{aligned}$$

valores autem coefficientium  $\alpha, \beta, \gamma, \delta$ , etc sunt sequentes

$$\alpha = n$$

$$\beta = n^2 + n$$

$$\gamma = n^3 + 4n^2 + n$$

$$\delta = n^4 + 11n^3 + 11n^2 + n$$

$$\epsilon = n^5 + 26n^4 + 66n^3 + 26n^2 + n$$

etc.

cujus progressionis legem facile inspicies. En igitur tria hujus generis Theoremata, quae singula certis casibus eximiam habebunt utilitatem ad summas serierum indagandas.

Quod deinde attinet ad summationes hujusmodi serierum, quae continentur in hac

$$1 + \frac{1}{2^n} + \frac{1}{3^n} + \frac{1}{4^n} + \frac{1}{5^n} + \text{etc.}$$

existente  $n$  numero pari eas duplici operatione sum consecutus, quarum alteram uti recte conjectus Vir Celeb. deduxi ex serie  $1 + \frac{1}{3^n} + \frac{1}{5^n} + \frac{1}{7^n} + \text{etc.}$  altera vero immediate mihi illius summam praebuit. Priore modo utique summas etiam hujusmodi serierum  $1 - \frac{1}{3^n} + \frac{1}{5^n} - \frac{1}{7^n} + \frac{1}{9^n} - \text{etc.}$  existente  $n$  numero impare detexi, invenique eas se habere, prorsus ac Tu indicas. Sunt autem summae tam pro paribus quam imparibus exponentibus  $n$  sequentes

$$\frac{p}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \text{etc.}$$

$$\frac{p^2}{8} = 1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \frac{1}{9^2} + \text{etc.}$$

$$\frac{p^3}{32} = 1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} + \frac{1}{9^3} - \text{etc.}$$

$$\frac{p^4}{96} = 1 + \frac{1}{3^4} + \frac{1}{5^4} + \frac{1}{7^4} + \frac{1}{9^4} + \text{etc.}$$

$$\frac{5p^5}{1536} = 1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} + \frac{1}{9^5} - \text{etc.}$$

$$\frac{p^6}{960} = 1 + \frac{1}{3^6} + \frac{1}{5^6} + \frac{1}{7^6} + \frac{1}{9^6} + \text{etc.}$$

$$\frac{61p^7}{194320} = 1 - \frac{1}{3^7} + \frac{1}{5^7} - \frac{1}{7^7} + \frac{1}{9^7} - \text{etc.}$$

$$\frac{17p^8}{161280} = 1 + \frac{1}{3^8} + \frac{1}{5^8} + \frac{1}{7^8} + \frac{1}{9^8} + \&c$$

etc.

quae series omnes continentur in una hac generali :

$$1 + (-\frac{1}{3})^n + (+\frac{1}{5})^n + (-\frac{1}{7})^n + (+\frac{1}{9})^n + \text{etc.}$$

existente  $n$  numero integro. Si enim  $n$  est numerus par, tunc

omnes termini habebunt signum + ; sin autem  $n$  sit impar, tum signa sese alternatim insequentur.

Omnes autem has summas derivavi ex hac aequatione infinita;

$$0 = 1 - \frac{s}{1 \cdot a} + \frac{s^3}{1 \cdot 2 \cdot 3 \cdot a} - \frac{s^5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot a} + \text{etc.}$$

qua relatio inter arcum  $s$  ejusque sinum  $a$  exprimitur in circulo cujus radius est 1. Quoniam igitur eidem sinui  $a$  innumerabiles arcus  $s$  respondent, necesse est. Si  $s$  consideretur tanquam radix istius aequationis, eam habituram esse infinitos valores, eosque omnes ex circuli indole cognitos. Sint ergo  $A, B, C, D$ , etc. omnes illi arcus, quorum idem est sinus  $a$  erit ex natura aequationum

$$\begin{aligned} 1 - \frac{s}{1 \cdot a} + \frac{s^3}{1 \cdot 2 \cdot 3 \cdot a} - \frac{s^5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot a} + \text{etc.} \\ = \left(1 - \frac{s}{A}\right) \left(1 - \frac{s}{B}\right) \left(1 - \frac{s}{C}\right) \text{etc.} \end{aligned}$$

Posita nunc ista fractionum serie ex omnibus illis arcibus formata  $\frac{1}{A}, \frac{1}{B}, \frac{1}{C}, \frac{1}{D}$  etc. perspicuum est summam hanc fractionum aequari coefficienti ipsius  $-s$  qui est  $= \frac{1}{a}$ ; seu fore  $\frac{1}{a} = \frac{1}{A} + \frac{1}{B} + \frac{1}{C} + \frac{1}{D} + \text{etc.}$  Simili modo summa factorū ex binis fractionibus aequatur coefficienti ipsius  $s^2$  qui est  $= 0$ . unde erit

$$0 = \frac{1}{2} \left( \frac{1}{A} + \frac{1}{B} + \frac{1}{C} + \text{etc} \right)^2 - \frac{1}{2} \left( \frac{1}{A^2} + \frac{1}{B^2} + \frac{1}{C^2} + \text{etc} \right),$$

seu 
$$\frac{1}{a^2} = \frac{1}{A^2} + \frac{1}{B^2} + \frac{1}{C^2} + \frac{1}{D^2} + \text{etc.}$$

Porro summa factorū ex ternis fractionibus aequalis esse debet coefficienti ipsius  $-s^3$ , qui est  $= -\frac{1}{6a}$ , unde deducitur summa cuborū illarum fractionū,

$$\frac{1}{A^3} + \frac{1}{B^3} + \frac{1}{C^3} + \frac{1}{D^3} + \text{etc} = \frac{1}{a^3} - \frac{1}{2a};$$

atque ita procedendo summae reperientur omnium serierum in hac generali  $\frac{1}{A^n} + \frac{1}{B^n} + \frac{1}{C^n} + \frac{1}{D^n} + \text{etc.}$  comprehensarum dummodo pro  $n$  sumatur numerus integer affirmativus. Si nunc pro sinu indefinito  $a$  ponatur sinus totus 1, illae ipsae oriuntur series quas Tecum communicavi. In istis autem summis notari meretur insignis affinitas inter coefficientes numericos harum summarū, atque terminos superioris progressionis, quam primum ad series quascunque summandas dedi, nempe hujus

$$\int X dx + \frac{X}{1.2} + \frac{dX}{1.2.3.2 dx} - \text{etc.}$$

Quo autem haec affinitas clarius perspiciatur, summas ipsas congruo modo expressas repraesentare visum est.

$$\frac{2^1.1}{1.2.3.2} p^2 = 1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \frac{1}{5^2} + \text{etc.}$$

$$\frac{2^3.1}{1.2.3.4.5.6} p^4 = 1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4} + \frac{1}{5^4} + \text{etc.}$$

$$\frac{2^5.1}{1.2.3.4.5.6.7.6} p^6 = 1 + \frac{1}{2^6} + \frac{1}{3^6} + \frac{1}{4^6} + \frac{1}{5^6} + \text{etc.}$$

$$\frac{2^7.3}{1.2.3 \dots 9.10} p^8 = 1 + \frac{1}{2^8} + \frac{1}{3^8} + \frac{1}{4^8} + \frac{1}{5^8} + \text{etc.}$$

$$\frac{2^9.5}{1.2.3 \dots 11.6} p^{10} = 1 + \frac{1}{2^{10}} + \frac{1}{3^{10}} + \frac{1}{4^{10}} + \frac{1}{5^{10}} + \text{etc.}$$

$$\frac{2^{11}.691}{1.2.3 \dots 13.210} p^{12} = 1 + \frac{1}{2^{12}} + \frac{1}{3^{12}} + \frac{1}{4^{12}} + \frac{1}{5^{12}} + \text{etc.}$$

$$\frac{2^{13}.35}{1.2.3 \dots 15.2} p^{14} = 1 + \frac{1}{2^{14}} + \frac{1}{3^{14}} + \frac{1}{4^{14}} + \frac{1}{5^{14}} + \text{etc.}$$

$$\frac{2^{15}.3617}{1.2.3 \dots 17.30} p^{16} = 1 + \frac{1}{2^{16}} + \frac{1}{3^{16}} + \frac{1}{4^{16}} + \frac{1}{5^{16}} + \text{etc.}$$

$$\frac{2^{17}.43867}{1.2.3 \dots 19.42} p^{18} = 1 + \frac{1}{2^{18}} + \frac{1}{3^{18}} + \frac{1}{4^{18}} + \frac{1}{5^{18}} + \text{etc.}$$

$$\frac{2^{19}.3222277}{1.2.3 \dots 21.110} p^{20} = 1 + \frac{1}{2^{20}} + \frac{1}{3^{20}} + \frac{1}{4^{20}} + \frac{1}{5^{20}} + \text{etc.}$$

etc.

Hac scilicet convenientia animadversa mihi ulterius progredi licuit, quam si methodo genuina inveniendi coefficientes potestati ipsius  $p$ , usus fuisset quippe qua labor nimis evaderet operosus. Quamobrem non dubito, quin nexu hoc mirabili penitus cognito (mihi enim adhuc sola constat observatione) praeclara adjumenta ad Analyseos promotionem sese sint proditura. Tu forte Vir Celeb. non difficulter nexum hunc ex ipsa rei natura derivabis.

Dum haec scribo, accipio a Cel. Nicolao Bernoulli Prof. Juris Basiliensi et Membro Societatis Vestrae singularem demonstrationem summae hujus seriei  $1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \text{etc.}$  quam deducit ex summa hujus notae  $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \text{etc.}$  illam considerans tanquam hujus quadratum minutum duplis factis binorū terminorum. Haec autem dupla facta seorsim contemplans multiferiam transformat, tandemque ad seriem quandam regularem perducit, quam analytice ostendit pariter a Circuli quadratura pendere. Sed hac methodo certe Viro Acutissimo non licuisset ad summas altiorum potestatum pertingere.

Eodem incommodo quoque laborat alia quaedam methodus mea, qua directe per solam analysin hujus seriei summam  $1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \text{etc.}$  inveni, ex qua pariter nullam utilitatem ad sequentes series summandas sum consecutus. Haec autem methodus ita se habet: Fluentem hujus fluxionis

$\frac{\dot{x}}{\sqrt{1-xx}}$ , qua arcus circuli exprimitur ejus sinus est  $= x$  existente sinu toto  $= 1$ , multiplico per ipsam fluxionem

$\frac{\dot{x}}{\sqrt{1-xx}}$ , quo prodeat facti fluens  $= \frac{1}{2}ss$ , posito  $s$  pro arcu illo ejus sinus est  $= x$  Si ergo post summationem peractam ponatur  $x = 1$ , fiet  $s = \frac{p}{2}$ , denotante  $p$  ad 1 rationem peripheriae ad diametrum; ita ut hoc casu habeatur  $\frac{pp}{8}$ . Fluens autem ipsius  $\frac{\dot{x}}{\sqrt{1-xx}}$  per seriem est

$$= x + \frac{1}{2 \cdot 3} x^3 + \frac{1 \cdot 3}{2 \cdot 4 \cdot 5} x^5 + \frac{1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6 \cdot 7} x^7 + \text{etc.}$$

Ducantur nunc singuli termini in fluxionem  $\frac{\dot{x}}{\sqrt{(1-xx)}}$  et sumantur fluentes ita ut fiant  $= 0$  posito  $x = 0$ , tum vero ponatur  $x = 1$ . Ita reperietur  $\int \frac{x\dot{x}}{\sqrt{(1-xx)}} = 1 - \sqrt{(1-xx)} = 1$ , posito  $x = 1$ .

Simili modo erit  $\frac{1}{2 \cdot 3} \int \frac{x^3 \dot{x}}{\sqrt{(1-xx)}} = \frac{1}{3 \cdot 3}$ ;

atque  $\frac{1 \cdot 3}{2 \cdot 4 \cdot 5} \int \frac{x^5 \dot{x}}{\sqrt{(1-xx)}} = \frac{1}{5 \cdot 5}$

et ita porro, adeo ut tandem obtineatur

$$\frac{\rho^2}{8} = 1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \text{etc.}$$

Sed huic argumento jam nimium sum immoratus, quocirca Te rogo Vir Celeb. ut quae Ipse hac de re es meditatus, mecum benevole communicare velis.

Incidit aliquando in hanc expressionem notatu satis dignam :

$$\frac{3 \cdot 5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 37 \cdot 41}{4 \cdot 4 \cdot 8 \cdot 12 \cdot 12 \cdot 16 \cdot 20 \cdot 24 \cdot 28 \cdot 32 \cdot 36 \cdot 40} \text{ etc.}$$

cujus numeratores sunt omnes numeri primi naturali ordine sese insequentes, denominatores vero sunt numeri pariter pares unitate distantes a numeratoribus. Hujus vero expressionis valorem esse aream circuli cujus diameter est  $= 1$ , demonstrare possum. Quamobrem haec expressio aequalis erit huic Wallisianae

$$\frac{2 \cdot 4 \cdot 4 \cdot 6 \cdot 6 \cdot 8 \cdot 8 \cdot 10 \cdot 10 \text{ etc}}{3 \cdot 3 \cdot 5 \cdot 5 \cdot 7 \cdot 7 \cdot 9 \cdot 9 \cdot 11 \text{ etc.}}$$

Ut autem novi quiddam Tibi Vir Celeb. perscribam Tuoque acutissimo subijciam judicio, communicabo quaedam problemata, quae inter Viros Celeberrimos Bernoullios et me ab aliquo tempore sunt versata. Proponebatur autem mihi inter alia problemata hoc, ut inter omnes curvas iisdem terminis contentas investigarem eam, in qua  $\int r^m s$  haberet valorem minimum, denotante  $s$  curvae arcum, et  $r$  radium curvaturae, quod problema ope consuetarū methodorum, quales Bernoulli,

Hermannus et Taylorus Vester dedere, resolvi non potest, quia in  $r$  fluxiones secundae ingrediuntur. Inveni autem jam ante methodum universalem omnia hujusmodi problemata solvendi, quae etiam ad fluxiones cujusque ordinis extenditur, ejus ope pro curva quaesita sequentem dedi aequationem  $a^m x + b^m y = (m+1) \int r^m \dot{s}$  in qua  $x$  et  $y$  coordinatas orthogonales hujus curvae denotant. Hinc autem sequitur casu, quo  $m = 1$ , cycloidem quaestioni satisfacere.

Deinde etiam quaerebatur inter omnes tantum curvas ejusdem longitudinis, quae per duo data puncta duci possunt ea, in qua  $\int r^m \dot{s}$  esset minimum; hancque curvam deprehendi ista aequatione indicari  $a^m x + b^m y + c^m s = (m+1) \int r^m \dot{s}$ .

Practerea quaerebantur etiam oscillationes seu vibrationes laminae elasticae parieti firmo altero termino infixae, cui quaestioni ita satisfeci, ut primo curvam, quam lamina inter vibrandum induit, determinarem, atque secundo longitudinem penduli simplicis isochroni definirem, quod aequalibus temporibus oscillationes suas absolvat; hinc enim intelligitur quot vibrationes data lamina dato tempore sit absolutura.

Ego vero contra inter alia problema istud proposui, ut inveniantur super dato axe duae curvae algebraicae non rectificabiles, sed quarum rectificatio a datae curvae quadratura pendeat, quae tamen arcuum eidem abscissae respondentium summam habeant ubique rectificabilem; ejus problematis difficillimi visi, neque a Bernoullio soluti, sequentem adeptus sum solutionem. Posita abscissa utrique curvae communi  $= x$ ; sit alterius curvae applicata  $= y$ ; alterius vero  $= z$ . Assumatur nova variabilis  $u$  ex qua et constantibus variables  $x$ ,  $y$  et  $z$  definiri debent, atque exprimat  $\int V \dot{u}$  illam quadraturam, a qua rectificatio utriusque curvae pendere debet; sintque  $p$  et  $q$  quantitates quaecunque algebraicae ex  $u$  et constantibus compositae. Quibus pro lubitu sumtis fiat

$$\sqrt[4]{(1+pp)} + \sqrt[4]{(1+qq)} = r : \sqrt[4]{(1+pp)} - \sqrt[4]{(1+qq)} = s,$$

tum quaerantur sequentes valores

$$A = \frac{\dot{q}}{\dot{p}}; \quad B = \frac{\dot{r}}{\dot{p}}; \quad C = \frac{\dot{s}}{\dot{p}}$$

item 
$$D = \frac{\dot{B}}{\dot{A}}; \quad E = \frac{\dot{C}}{\dot{A}} \quad \text{et} \quad F = \frac{\dot{E}}{\dot{D}}.$$

Ex his quantitativibus porro formentur istae

$$P = \frac{V\dot{u}}{\dot{F}}; \quad Q = \frac{\dot{P}}{\dot{D}}; \quad \text{et} \quad R = \frac{\dot{Q}}{\dot{A}}.$$

Ex his denique valoribus, qui omnes erunt algebraici sumta communi abscissa  $= \frac{\dot{R}}{\dot{P}},$

fiat 
$$y = \frac{p\dot{R}}{\dot{P}} - R \quad \text{atque} \quad z = \frac{q\dot{R} - R\dot{q}}{\dot{P}} + Q;$$

hacque ratione, cum  $p$  et  $q$  sint quantitates arbitrariae problemati infinitis modis satisfieri poterit. Erunt enim ambae curvae algebraicae, atque utriusque rectificatio pendebit a fluente hujus fluxionis  $V\dot{u}$ . Summa vero amborū arcuum algebraice exprimi poterit. Est enim summa arcuum

$$= rx - BR + DQ - P$$

differentia vero eorum est

$$= sx - CR + EQ - FP + \int V\dot{u}.$$

Detexi autem pro resolutione hujusmodi problematum peculiarem methodum, quam Analysin infinitorū indeterminatam appellavi, atque jam maximam partem in singulari tractatu exposui. At tam longam epistolam scribendo vereor ne patientiam Tuam nimis fatigem: quamobrem rogo, ut prolixitati meae veniam des, eamque tribuas summae Tui existimationi, quam jamdudum concepi. Vale Vir Celeberrime, meque uti coepisti amicitia Tua dignari perge.

dabam Petropoli

ad d. 27 Julii 1738.

## XII

### FOLKES AND STIRLING

(1)

*Folkess to Stirling, 1747*

Dear Sir

After so many years absence I am proud of an opportunity of assuring you of my most sincere respect and good wishes for your prosperity and happiness of all sorts. I received the day before yesterday of a Gentleman just arrived from Berlin, the enclosed Diploma which I am desired to convey to you with the best respects of the Royal Academy of Sciences of Prussia, and more particularly of M<sup>r</sup> de Maupertuis the President and M<sup>r</sup> de Formey the Secretary. M<sup>r</sup> Mitchell going your way I put it into his hands for you and congratulate you Sir upon this mark of the esteem of that Royal Academy upon their new establishment under their present President. Our old friend M<sup>r</sup> Montagu is well and we often talk of you together, and our old Master de Moivre whom we dined with the other day on the occasion of his compleating his eightieth year. I remain with the truest esteem and affection

Dear Sir

Your most obedient humble servant

London June 10. 1747

M. FOLKES. Pr. R.S.

member of the Royal Academies  
of Sciences of Paris and Berlin,  
and of the Society of  
Edinburgh

M<sup>r</sup> Stirling

# NOTES

## UPON THE CORRESPONDENCE

### I

MACLAURIN (1698-1746), F.R.S. 1719

Colin Maclaurin was born at Kilmodan in Argyleshire, and attended Glasgow University. He became Professor of Mathematics at Aberdeen in 1717, and in 1725 was appointed to the chair of Mathematics in Edinburgh University. He died in 1746.

His published works are *Geometria Organica*, 1720; *Treatise of Fluxions*, 1742; *Treatise of Algebra*, 1748, and an *Account of Newton's Philosophical Discoveries*, 1748.

His *Treatise of Fluxions*, which made a suitable reply to the attack by Berkeley, also gives an account of his own important researches in the Theory of Attraction.

#### *The Dispute between Maclaurin and Campbell.*

Letters I. 1 to I. 7 are mainly concerned with a dispute between Colin Maclaurin and George Campbell, a pretty full account of which is given in Cantor's *Geschichte der Mathematik*.

But the correspondence before us gives a good deal of fresh information, as well as practically the only details known regarding George Campbell, about whom the Histories of the Campbell Clan are silent, in spite of the fact that he was a Fellow of the Royal Society, being elected in 1730. From Letter I. 1, it would appear that when Maclaurin, glad to leave Aberdeen University owing to the friction arising from his absence in France, and consequent neglect of his professorial duties, accepted the succession to Professor Gregory in the Chair at Edinburgh, he had in a sense stood in the way of Campbell for promotion to the same office. Feeling this, he had done his best to advance Campbell's interests otherwise and had corresponded to this intent with Stirling, who

suggested that Campbell might gain a livelihood in London by teaching. Some of Campbell's papers were sent to London. One, at least, was read before the Royal Society, and, through the influence of that erratic genius, Sir A. Cuming, ordered to be printed in the Transactions. Stirling himself read the paper in proof for the Society. When the paper appeared Maclaurin was much perturbed to find that it contained some theorems he had himself under discussion as a continuation of his own on the Impossible Roots of an Equation.

He wrote letters to Folkes explaining his position, and giving fresh additional theorems. But the matter did not end here. For Campbell in a jealous mood wrote and published an attack upon Maclaurin, who found himself compelled to make a similar public defence. An attempt was also made to embroil Stirling with Maclaurin, fortunately without success. Practically nothing further is known regarding George Campbell (who is not to be confused with Colin Campbell, F.R.S., of the Jamaica Experiment, mentioned in Letter I. 10). The names of G. Campbell and Sir A. Cuming are given in the list of subscribers to the *Miscellanea Analytica de Seriebus* of De Moivre (1730).

*Newton's Theorem regarding the nature of the Roots  
of an Algebraic Equation.*

Neither Campbell nor Maclaurin attained the object aimed at,—to furnish a demonstration of Newton's Theorem, stated without proof in the *Arithmetica Universalis*.

Other as eminent mathematicians were to try and fail, and it was not until the middle of the nineteenth century that a solution was furnished by Sylvester, who also gave a generalization. (*Phil. Trans.* 1864: *Phil. Mag.* 1866.)

Newton's Theorem may be stated thus (vide Todhunter's *Theory of Equations*).

Consider the equation

$$f(x) = a_0 x^n + {}_nC_1 a_1 x^{n-1} + \dots + {}_nC_r a_r x^{n-r} + \dots + a_n = 0.$$

Form the two rows of quantities

$$\begin{array}{ccccccc} a_0 & a_1 & a_2 & \dots & a_n \\ A_0 & A_1 & A_2 & \dots & A_n \end{array}$$

where

$$A_0 = a_0^2; \dots A_r = a_r^2 - a_{r-1} a_{r+1}; \dots A_n = a_n^2.$$

Call

$$a_r \quad a_{r+1}$$

$$A_r \quad A_{r+1}$$

an associated couple of successions. In such a couple the signs of  $a_r$  and  $a_{r+1}$  may be alike and represent a Permanence,  $P$ ; or unlike, and represent a Variation  $V$ .

Similarly for  $A_r$  and  $A_{r+1}$ .

An associated couple may thus give rise to

- (1) a double Permanence,
- (2) a Permanence-Variation,
- (3) a Variation-Permanence,
- (4) a double Variation.

Then we have Newton's Rule:—

The number of double Permanences in the series of couples is a superior limit of the number of negative roots; and the number of Variation-Permanences is an upper limit of the positive roots; so that the number of Permanences in the Series

$$A_0 \quad A_1 \dots A_n$$

is an upper limit to the number of the real roots of  $f(x) = 0$ .

Sylvester (v. *Collected Works*) was the first to furnish a demonstration of Newton's Theorem, and he gave the following generalization.

Write  $f(x + \lambda)$  in the form

$$a_0 x^n + a_1 x^{n-1} + \dots + a_n = 0$$

and form the table

$$\begin{array}{cccc} a_0 & a_1 & a_n & \\ A_0 & A_1 & A_n & \end{array}$$

(where  $A_0, \dots, A_n$  are as before).

Denote the number of double Permanences arising therefrom by  $PP(\lambda)$ .

Similarly denote by  $PP(\mu)$  the number corresponding to  $f(x + \mu)$ .

Then if  $\mu > \lambda$ ,  $PP(\mu) - PP(\lambda)$  is either equal to the

number of real roots of  $f(x) = 0$  between  $\mu$  and  $\lambda$ , or exceeds it by an even number.

#### Letter I. 1.

On p. 19 of his *Défence* (against Campbell) Maclaurin makes the statement:—

‘In a *Treatise of Algebra*, which I composed in the Year 1726, and which, since that Time, has been very publick in this Place, after giving the same Demonstration of the Doctrine of the *Limits*, as is now published in my second Letter, I add in Article 50 these Words, &c.’

Maclaurin appears to be referring here to a course of lectures to his students.

Maclaurin’s *Algebra* did not appear until 1748, after his death. It was in English, but contained an important appendix in Latin on the *Properties of Curves*. De Moivre’s book referred to is his *Miscellanea Analytica*, 1730. In 1738 appeared the second edition of his *Doctrine of Chances*, also referred to in the letters.

#### Letter I. 3.

This letter, dated by Maclaurin as February 11<sup>th</sup>, 1728, should have been dated as February 11<sup>th</sup>, 172 $\frac{8}{9}$ , i.e. 1728 Old Style, or 1729 New Style.

Stirling makes this correction in I. 6, which consists of extracts from letters by Maclaurin. Until this had been noted, the first three letters seemed hopelessly confused. Maclaurin shows the same slovenliness in the important note of his, I. 10, attached to the letter from Maupertuis to Bradley.

#### Letter I. 6.

Letter I. 6 contains only extracts from letters of Maclaurin, including one dated October 22, 1728, which is no longer in the Stirling collection.

#### Letter I. 7.

In the spring of 1921 I had the good fortune to obtain a copy of Maclaurin’s reply to Campbell.

It is entitled:—

‘A **Defence** of the **Letter** published in the Philosophical Transactions for March and April 1729, concerning the **Impossible Roots** of Equations; in a Letter from the Author to a Friend at London.

Qui admonent amice, docendi sunt: qui inimice infectantur, repellendi.

Cicero \*

The name of the ‘Friend’ is not given. The ‘Defence’ consists of twenty small quarto pages, and contains numerous extracts from the letters to Stirling; and towards the end Campbell’s statements regarding Maclaurin’s theorems are refuted.

Campbell is generally referred to as ‘the **Author** of the **Remarks**’ (on Maclaurin’s Second Letter on impossible roots); though also as ‘the **Remarker**’.

Maclaurin gives the extract from the letter of October 1728 (cf. I. 6), and adds:—

‘See the 2d and 3d *Examples* of the *Eighth Proposition* of the *Lineae tertii Ordinis Newtonianae*.’

There is also the following passage containing an extract from a letter by Stirling, not otherwise known:—

‘I had an Answer from this Gentleman in *March*, from which, with his Leave, I have transcribed the following Article:

“I shewed your Letter (*says he*) to Mr *Machin*, and we were both well satisfied that you had carried the Matter to the greatest Height, as plainly appears by what you have said in your Letter. But it is indeed a Misfortune, that you was so long in giving us the Second Part, after you had delivered some of your *Principles* in the First:—Since you have published Part of your Paper before Mr C——ll, and now have the rest in such Readiness, I think you have it in your Power to do yourself Justice more than any Body else can. I mean by a speedy Publication of the remaining Part: For I am sure, if you do that, there is no *Mathematician*, but who must needs see, That it is your own Invention, after the Result of a great Deal of Study that way.”

I received this Letter in *March*, and, in consequence of this

kind Advice, resolved to send up my Second Paper as soon as possible.'

Maclaurin makes it clear that he had not intended his First Letter to Folkes to be published. It was printed without his knowledge. Had he known in time, he would have deferred its publication until he had more fully investigated additional theorems which he had on the same subject; and he gives an extract from a letter from Folkes in corroboration of his statement.

#### Letter I. 8.

Letter I. 8 is reproduced because of its reference to an office (in the Royal Society) for which Stirling had been thought fit.

#### Letter I. 9.

Letter I. 9 announces that Maclaurin has started to write his *Treatise of Fluxions*. His conscientious reference to original authorities has been noted by Reiff (*Geschichte der Unendlichen Reihen*). The earlier proof-sheets of the Treatise, at least, passed through Stirling's hands.

These facts bear interesting evidence regarding the *Euler-Maclaurin Summation Formula*, to which I have to return in connexion with the correspondence between Stirling and Euler in Letters XI.

Simpson, referred to by Maclaurin, is doubtless his old teacher, Robert Simson, of Glasgow University.

#### Letter I. 10.

Letter I. 10, which is a mere scrawl written on the outside of the copy of the letter from Maupertuis to Bradley, is of interest in the history of the Royal Society of Edinburgh, and is to be associated with the two letters of Maclaurin published in the *Scots Magazine* for June, 1804.

The date of the letter of Maupertuis shows that Maclaurin should have given Feb. 4<sup>th</sup>, 1737 $\frac{7}{8}$  as the date of his own.

Maclaurin was more successful with Stirling than with R. Simson, who refused to become a member after Maclaurin had got him nominated. (*Scots Mag.*)

Bradley's translation of the letter of Maupertuis is reproduced in the *Works and Correspondence of Bradley*, 1832

(Rigaud). The original French letter is preserved in one of the letter books of the Royal Society of London.

*Foundation of the Philosophical Society of Edinburgh.*

Letter I. 10 confirms the date of foundation as 1737 (v. Forbes's *History of the Royal Society of Edinburgh*, in General Index Trans. R.S.E. published 1890).

But at the date of this letter I. 10 the Society was not complete in numbers, for Stirling was not yet a member.

By 1739 the Society had outrun its original bounds, having forty-seven members whose names are given (p. 26 of Gen. Index Trans. R.S.E.).

More or less informal meetings were held in 1737. Maclaurin and Dr. Plummer, Professor of Chemistry in the University, were the Secretaries. The Rebellion of 1745 seriously affected the activity of the Society, and Maclaurin's death in 1746 was also a severe blow.

The papers read before the Society had been in Maclaurin's hands, but only some of these were found. Three volumes of *Essays and Observations, Physical and Literary* (dated 1754, 1756, 1771), were published. The papers in Vol. I are not in chronological order, but those by Plummer are fortunately dated, the first bearing the date January 3, 1738. Dr. Pringle, afterwards President of the Royal Society of London, followed in February. Then it was Maclaurin's turn in March, when he gave two papers, one being on the Figure of the Earth (*Scots Magazine*).

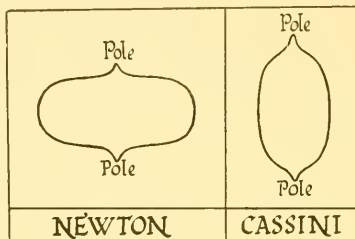
These two papers are not printed in the *Essays*, &c. But among the Maclaurin MSS. preserved in Aberdeen University there is one entitled 'An Essay on the Figure of the Earth'.

On the foundation of the Royal Society of Edinburgh in 1783 the members of the Philosophical Society were assumed as Fellows. Maclaurin's son John (Lord Dreghorn) is one of those mentioned in the original charter of the Royal Society.

*Letter of Maupertuis.*

The letter of Maupertuis must have given lively satisfaction to Maclaurin and Stirling. Newton had assumed as a postulate that the figure of the Earth is approximately that of an oblate spheroid, flatter at the poles than at the Equator. The

Cassinis, arguing from measurements of the arc of a Meridian in France, maintained that the figure was that of a prolate spheroid. There were thus two hostile camps, the Newtonians and the Cassinians.



The French expedition to Lapland (1736-7) with Maupertuis as leader, and Clairaut as one of the party, conclusively established the accuracy of Newton's hypothesis. In the words of Voltaire, Maupertuis had 'aplati les Poles et les Cassinis'.

Both Stirling and Maclaurin made important contributions to the subject, and the rest of the letters preserved as passing between them refer mainly to their researches on Attraction and on the Figure of the Earth.

Readers who are interested cannot do better than consult Todhunter's *History of the Theory of Attraction and of the Figure of the Earth* for full details. The letters, however, clear up some difficulties that were not always correctly explained by Todhunter.

#### Letter I. 11.

The Dean, near Edinburgh, Maclaurin's new address, now forms a residential suburb of Edinburgh.

De Moivre's book is doubtless the second edition of the *Doctrine of Chances* (1738).

#### Letter I. 13.

The remark made by Stirling towards the conclusion that 'the gravitation of the particle to the whole spheroid will be found to depend on the quadrature of the circle' seems to have given Maclaurin a good deal of trouble (cf. I. 14).

Maclaurin's reference to it in his *Fluxions*, § 647, as due to Stirling, was inexplicable to Todhunter, as Stirling never published his theorem. But Todhunter's conjecture (*History*, vol. i, p. 139) that Maclaurin may have inadvertently written Stirling for Simpson is of course quite a mistake.

#### Letter I. 15.

Compare the correspondence with Machin IX, Clairaut X, and Euler XI.

#### Letter I. 16.

This letter, dated 1740, furnishes ample justification of Todhunter's contention that the researches of Maclaurin, 'the creator of the theory of the attraction of ellipsoids', are quite independent of those given by T. Simpson in his *Mathematical Dissertations* (1743). Simpson lays claim to priority in certain theorems of the Fluxions on the ground that these given by himself were read before the Royal Society in 1741.

The *Treatise of Fluxions* so near completion in 1740 was not published until 1742.

## II

### CUMING

Sir A. Cuming (1690?–1775) was the only son of Sir Alexander Cuming, M.P., the first baronet of Culter, Aberdeen. Cuming went to the Scotch bar, but gave up his profession on receiving a pension. In 1720 he became a Fellow of the Royal Society. Though no mathematical writings of his are known, he seems to have been possessed of mathematical ability. He was on friendly terms with De Moivre and Stirling, both of whom acknowledge their indebtedness to him for valuable suggestions. At Aberdeen there is preserved a short letter (Nov. 3, 1744) from him to Maclaurin, in which he shows his interest in the controversy regarding Fluxions.

In his introduction to the *Methodus Differentialis*, Stirling speaks of him as 'Spectatissimus Vir'. Being a friend of Campbell he had a share in the dispute between Maclaurin and Campbell.

In 1729–30 he was in the American Colonies, visited the Cherokees, and became one of their chiefs. On his return to

England with some of the chiefs he was instrumental in arranging a treaty for his tribe.

Later he fell into poverty, and was confined in the Fleet prison from 1737 to 1765, losing his fellowship in the Royal Society for neglecting to pay his annual fee. In 1766 he obtained admission to the Charterhouse and died there in 1775.

### III

#### CRAMER AND STIRLING

Gabriel Cramer was born in 1704 in Geneva, where his father practised medicine. In 1724 he was, conjointly with Calandrini, entrusted with the instruction in Mathematics at the University of Geneva. In 1727 he started on a two years' tour, visiting Bâle, where he studied under John Bernoulli, and England, where he became acquainted with Stirling and De Moivre, and returning by Paris. He became F.R.S. in 1748. He died in 1752. He is best known through his *Introduction à l'Analyse des lignes courbes algébriques*. He also edited the works of James and John Bernoulli.

#### Letter III. 1.

It is unfortunate for us that Cramer did not discover before 1732 that he wrote 'un Anglois aussi barbare'.

Regarding the history of the Probability Problem in III. 1, see Todhunter's *History of the Theory of Probability* (p. 84). De Moivre gives a much simpler solution in the *Miscellanea Analytica* (1730).

#### Letter III. 2.

Compare Letter IV. 2 (Bernoulli).

#### Letter III. 3.

In this letter of introduction Cramer in the address describes Stirling as L.A.M. I do not know what these letters signify.

#### Letter III. 8.

Letter III. 8 contains valuable information regarding the manner in which Stirling wrote his *Methodus Differentialis*. The blank made for the formula given by De Moivre was never filled up: but the formula in question is of course easily

obtained from the *Supplement* to the *Miscellanea Analytica* of De Moivre. We have also the important information that this *Supplement* appeared *after* the publication of Stirling's own Treatise.

Letter III. 10.

One will note Cramer's difficulties with the graph of  $y^x = 1 + x$ ; also his determination of  $(1 + x)^{1/x}$  as  $x$  tends to zero.

It is a pity there is no indication of Stirling's determination of this limit.

*Stirling's Series*

*and the claims to priority of De Moivre and Stirling.*

In the *Bibliotheca Mathematica* for 1904 (p. 207) Eneström makes the following statement.

‘Im Anschluss an den Bericht über Stirling's Formel für die Summe einer Anzahl von Logarithmen wäre es angezeigt mitzuteilen dass die bekannte Formel dieser Art die man jetzt ziemlich allgemein gewohnt ist als die Stirlingsche Formel zu bezeichnen, nämlich

$$\log(1 \cdot 2 \cdot 3 \dots x) = \frac{1}{2} \log 2\pi + (x + \frac{1}{2}) \log x - x + A_2 \frac{1}{x} + A_4 \frac{1 \cdot 2}{x^3} + \&c.,$$

zuerst von Moivre im Anhang an der *Misc. analytica* (1730) angegeben und hergeleitet wurde. Moivre berichtet selbst dass Stirling ihm brieflich die Formel

$$\log(1 \cdot 2 \dots x) = \frac{1}{2} \log 2\pi + (x + \frac{1}{2}) \log(x + \frac{1}{2}) - (x + \frac{1}{2}) - \frac{1}{2 \cdot 12 (x + \frac{1}{2})} + \frac{7}{8 \cdot 360 (x + \frac{1}{2})^3} - \dots$$

mitgeteilt hatte, und dass er selbst dadurch angeregt wurde die neue Formel auf einem ganz anderen Wege aufzufinden.’

Inasmuch as the only change effected by De Moivre is to give the expansion of  $\log(x!)$  in descending powers of  $x$  instead of descending powers of  $x + \frac{1}{2}$ , which has no special advantage when  $x$  is large, the priority of De Moivre to this important formula seems to me to rest on very slender foundations, unless we are to infer from Eneström's reference to the

*Supplement to the Miscellanea Analytica* that De Moivre published his result prior to Stirling.

Eneström's statement has had considerable influence with subsequent writers (e.g. Czuber and Le Roux, *Calcul des Probabilités*; Selivanov and Andoyer, *Calcul des Différences Finies*, in the well-known *Encyc. des Sciences Math.*; Czuber, *Wahr. Rechnung*, 1903, s. 19), who refer for proof to the *Supp. Misc. Anal.* of De Moivre.

Against these we may put De Moivre's own statement in the third edition of the *Doctrine of Chances* (1756), given in the Appendix, p. 334, where, after giving a table of values for  $\log(x!)$  for numerical values of  $x$  he goes on to add:—

‘If we would examine these numbers, or continue the Table farther on, we have that excellent Rule communicated to the Author by Mr *James Stirling*, published in his *Supplement to the Miscellanea Analytica*, and by Mr Stirling himself in his *Methodus Differentialis*, Prop. XXVIII.

‘Let  $z - \frac{1}{2}$  be the last term of any Series of the natural Numbers 1, 2, 3, 4, 5, ...  $z - \frac{1}{2}$ ;  $a = \cdot 43429448190325$  the reciprocal of Neper's Logarithm of 10: Then three or four terms of this Series

$$z \text{ Log } z - az - \frac{a}{2 \cdot 12z} + \frac{7a}{8 \cdot 360z^3} - \frac{31a}{32 \cdot 1260z^5} + \frac{127a}{128 \cdot 1680z^7} - \&c$$

added to 0·399089934179, &c. which is half the Logarithm of a Circumference whose Radius is Unity, will be the Sum of the Logarithms of the given Series; or the *Logarithm* of the Product

$$1 \times 2 \times 3 \times 4 \times 5 \dots \times z - \frac{1}{2} \&c.'$$

There is thus no doubt in De Moivre's mind that the discovery of the theorem in question is not due to himself but to his friend Stirling.

*Date of Supplement to the Miscellanea Analytica.*

At first sight the Supplement appears to bear the date Jan. 7, 17 $\frac{29}{30}$ . In such case it would almost certainly be anterior in publication to Stirling's book.

Now this supposition is quite erroneous. The *Miscellanea Analytica*, as originally published, bears the above date, and contains no supplement. (The first copy I consulted has no supplement.) An examination of a copy with the *Supplement* shows two lists of Errata, the first after p. 250, and the second after p. 22 of the *Supplement*, the latter list containing Errata observed by De Moivre and his friends 'post editum librum meum'.

The letter III. 8 of Stirling puts it beyond a doubt that the *Supplement* had not appeared at the time he wrote (September 1730), so that its appearance was posterior to the publication of Stirling's *Methodus Differentialis*.

We have thus the following events in chronological order.

De Moivre publishes the *Misc. Anal.* early in 1730. His friend Stirling points out to him the poor approximation he gives for  $\log(x!)$  when  $x$  is large and sends him a formula of much greater accuracy. Stirling publishes his *Meth. Diff.* containing the famous *Stirling Series*. In the meantime De Moivre busies himself with Stirling's formula, and obtains it in a slightly different form but by an entirely different process: and he publishes his result as a *Supplement* to his book and bound with it, but without changing the date of his book. He explains in his own garrulous way, which makes the reading of his works so attractive nowadays, how he had *very nearly* got at Stirling's Theorem before he had heard from Stirling.

Will any scholar be bold enough to assert that the theorem is due to De Moivre in virtue of this latter statement, published after Stirling had given the theorem in all its generality in the *Meth. Diff.*? You may speak of De Moivre's form of Stirling's Theorem if you please, but the merit of discovering a theorem of the kind seems to rest indisputably with Stirling.

#### IV

#### N. BERNOULLI AND STIRLING

Nicholas Bernoulli was born in 1687 at Bâle in Switzerland, his father being a merchant in that town. His two uncles, James Bernoulli (1654-1705) and John Bernoulli (1667-1748), were both noted mathematicians.

He studied first under the former at Pâle University, and then under the latter at Gröningen, returning with his uncle John to Bâle in 1705.

He devoted himself to the study of mathematics and law. He became F.R.S. in 1713. On the recommendation of Leibniz, he was in 1716 appointed Professor of Mathematics at Padua, resigning in 1719 and returning to Bâle. In 1722 he was elected to the chair of Logic, and in 1731 to the chair of Law in Bâle. He died in 1759.

His cousins, the sons of John<sub>1</sub>, Nicholas 1695–1726; Daniel 1700–82; and John<sub>2</sub> 1710–90 were also noted mathematicians. Two of the three sons of John<sub>2</sub>, viz. John<sub>3</sub> and James, also showed mathematical ability, so that we have here a remarkable instance of three generations of distinguished mathematicians in one family. Venice was a favourite resort of the Bernoullis about the time that Stirling resided there.

#### Letter IV. 1.

Letter IV. 1 is the earliest of the letters preserved in the mathematical correspondence of Stirling. When the acquaintance between Bernoulli and Stirling began is unknown, but Bernoulli in the course of his travels spent some time in Oxford in 1712, when Stirling was still an undergraduate. One is strongly tempted to suggest that it was at Oxford that they first met, for the disparity in their years was not very great, while the number of students of mathematical tastes cannot have been very large. The fact of Bernoulli's presence in Oxford I have discovered in the *Correspondance Math. et Physique*, edited by N. Fuss, vol. ii, p. 183, where, in a letter to Daniel Bernoulli, Goldbach makes the remark:—

‘Cum Oxonii agerem A. 1712, atque per unum alterumve diem communi diversorio uterer cum consobрино Tuo Cl. Nicolao Bernoullio, donavit me dissertatione quadam Jacobi Bernoulli de seriebus infinitis &c.’

(Lettre V Goldbach à D. Bernoulli, 4 Nov. 1723)

Incidentally we learn an interesting fact regarding Goldbach that has escaped the notice of M. Cantor, who, in the Vorwort to the second edition of his *Geschichte*, gives 1718 as the earliest date he has found in connexion with the travels of Goldbach.

Confirmation as far as N. Bernoulli is concerned is found on p. 300 of vol. ii of Brewster's *Life of Newton*. He (i.e. Bernoulli) went to London in the summer<sup>1</sup> of 1712, where he met with the kindest reception from Newton and Halley, a circumstance which he speaks of with much gratitude in a letter in which he thanks Newton for a copy of the second edition of the *Principia*. (Letter dated Padua, May 31, 1717.)

Query: Did Goldbach meet Newton?

### *Taylor's Problem.*

The problem sent by Taylor to Montmort was a challenge to the continental mathematicians:—

‘Problema analyticum omnibus geometris non Anglis propositum: Invenire per quadraturam circuli vel hyperbolae Fluentem hujus quantitatis

$$z^{\frac{\delta}{\lambda}n-1}/(e+fz^n+gz^{2n}).'$$

Taylor had obtained it in the posthumous papers of Cotes, who died in 1716, while his *Harmonia Mensurarum*, in which the solution is given, was not published until 1722. The limitation on  $\lambda$  was given by Taylor because only in such a case had Cotes effected a solution. The challenge was really intended for John Bernoulli.

John Bernoulli published a solution in May 1719 (Leip. Actis). Other solutions were given by Hermann, Professor of Mathematics at Padua,<sup>2</sup> and by Ganfredi. (Montucla.)

### IV. 4.

Letter IV. 4 is written in a typical Bernoullian spirit as a reply to Stirling's letter IV. 3. Bernoulli's letter, however, contains a number of valuable criticisms upon the two published works of Stirling on *Cubic Curves*, and on *Series*, to which Stirling would have had to give careful attention had second editions of his works ever been contemplated by him, and to which I may have to advert on another occasion.

For the present I restrict my attention to the discovery Bernoulli makes known of a new variety of cubic omitted by

<sup>1</sup> ‘Visit to England during the months of September and October 1712.’ (Edleston, note, p. 142.)

<sup>2</sup> Formerly.

both Newton and Stirling in their enumeration of Cubic Curves. (Newton's error, which Bernoulli points out, is retained in the Horsley edition.)

In the enumeration of the cubics given by the equation

$$xy^2 = Ax^2 + Bx + C$$

only four of the six possible species are enumerated by Newton, and by Stirling following Newton.

Of the two missing species, Nicole in 1731 gave one (an oval and two infinite branches) corresponding to

$$xy^2 = p^2(x + \alpha^2)(x + \beta^2)$$

or

$$xy^2 = -p^2(x - \alpha^2)(x - \beta^2).$$

N. Bernoulli here announces (in 1733) the discovery of another, consisting of an acnode and two infinite branches as given by the equation

$$xy^2 = \pm p^2(x \pm \alpha^2)^2.$$

Thus Bernoulli takes precedence of Stone 1736, Murdoch and De Gua 1740, to whom reference is made by W. W. R. Ball, in his valuable memoir on *Newton's Classification of Cubic Curves* (Trans. L.M.S. 1891).

Murdoch (*Newtoni Genesis Curvarum per Umbras*, p. 87) has the remark:—

‘Speciem hanc No VIII Analogam apud Newtonum desiderari animadverterat D. Nic. Bernoulli, quod me olim monuit D. Cramer, Phil. et Math. apud Genevenses celebris Professor.’

## V

### CASTEL

Louis Bertrand Castel (1688–1757), a Jesuit Father, was the author of *Le vrai système de Newton*. He became F.R.S. in 1730.

Stirling's letter V. 2 contains a clear exposition of what he understands by geometrical demonstration.

## VI

### CAMPAILLA

Thomas Campailla was born at Modica in Sicily in 1668, and died in 1740. He studied in succession law, astrology,

and philosophy, and finally devoted himself entirely to the Natural Sciences and Medicine. He was not a Fellow of the Royal Society.

## VII

## BRADLEY

J. Bradley, 1692-1762, was a distinguished Astronomer. Like Stirling, he studied at Balliol College, Oxford. He became F.R.S. in 1718. In 1721 he was appointed to the chair of Astronomy in Oxford, in succession to Keill. He succeeded Halley as Astronomer Royal in 1742. He discovered the aberration of the fixed stars and the nutation of the earth's axis.

Both the letters here given are to be found in Rigaud's *Bradley*. Stirling's letter is taken from Rigaud; and Bradley's reply is among the letters preserved at Garden.

## VIII

## KLINGENSTIERNA

S. Klingenstierna was Professor of Mathematics at Upsala. It was through Cramer that he was introduced to Stirling (cf. Letter III. 3). In view of his researches in Optics, the letter here given is of some interest. He became F.R.S. in 1730.

## IX

## JOHN MACHIN

John Machin, the astronomer, became F.R.S. in 1710 (the same year as Poleni, Professor of Astronomy at Padua, mentioned in the postscript to IV. 1), and acted as Sec. R.S. from 1718 to 1747. He sat on the committee appointed in 1712 to investigate the dispute between Newton and Leibniz. In 1713 he became Professor of Astronomy at Gresham College. He died in 1751.

Machin used the formula

$$\pi/4 = 4 \arctan \frac{1}{5} - \arctan \frac{1}{239}$$

to calculate  $\pi$  to 100 places of decimals. His result is given (1706) in Jones's *Synopsis Palmariorum Matheseos*, in which the symbol  $\pi$  is first used for the number 3.14159 . . .

His 'Laws of the Moon's Motion according to Gravity' is appended to Motte's translation of the *Principia*.

A greater work on Lunar Theory, begun in 1717, was never published: and relative manuscripts are in the possession of the Royal Astronomical Society.

#### Letter IX. 1.

In connexion with this letter, which has no date, see the letters from Bernoulli to Stirling, IV.

#### Letter IX. 2.

Machin was keenly interested in the researches of Maclaurin and Stirling concerning the Figure of the Earth, though his name does not appear to find a place in Todhunter's *History* of the subject.

The book by Maupertuis is probably one on the Figure of the Earth mentioned by Todhunter (vol. i, p. 72).

Machin, in speaking of Stirling's Proposition concerning the Figure of the Earth, cannot refer to Stirling's Memoir entitled 'Of the Figure of the Earth and the Variation of Gravity on the Surface', which appeared in the *Phil. Trans.* for 1735-6.

Compare Stirling's letter to Maclaurin I. 15, in which he refers to his correspondence with Machin.

I do not quite understand Machin in his reference to the invention of Euler's Series, though Stirling's letter, if it could be found, would explain.

By 1738 Stirling had got definitely settled as Manager of the Lead Hills Mines in Scotland. He had apparently complained to Machin how he felt the isolation from his scientific friends and their researches in London. Machin's letter to him is written in the kindest spirit of warm friendship.

The book of De Moivre mentioned in the letter is doubtless the second edition of the *Doctrine of Chances* (1738).

## X

### CLAIRAUT

Born at Paris in 1713. Clairaut showed a wonderful precocity for mathematics, and at eighteen years of age he

published his celebrated 'Recherches sur les Courbes à double Courbure'. He took part in the expedition to Lapland under Maupertuis to determine the length of the arc of the meridian. He made several contributions to the Theory of the Figure of the Earth, which he ultimately embodied in the classic work entitled *Théorie de la Figure de la Terre* (1743). His *Théorie de la Lune* appeared in 1765, shortly before his death. He was also the author of *Éléments de la Géométrie* (1741), and of an *Algèbre* (1746). He became F.R.S. in 1737. He died in 1765.

'Clairaut a eu pour élève et pour amie la célèbre Marquise de Châtelet, la docte et belle Émilie, qu'il a aidée dans sa traduction du Livre des principes' (Marie, *Hist. Math.*), a state of affairs not over-pleasing to Voltaire.

In the letter here given we find Clairaut introducing himself to Stirling. Cf. I. 15. Clairaut had frequent correspondence with Maclaurin, and several of the letters have been preserved.

## XI

### EULER

Leonhard Euler (1707-83) was born at Bâle in Switzerland. He studied Mathematics under John Bernoulli, having as fellow-students Nicholas and Daniel Bernoulli, the two sons of John Bernoulli. The two brothers were called to Petrograd in 1725, and Euler followed in 1727. In 1741, on the invitation of Frederick the Great, he went to Berlin, returning again in 1766 to Petrograd, where he died in 1783. For almost the whole of his second residence in Russia he was totally blind, but this misfortune had little effect on his wonderful production of mathematical memoirs. There is hardly a department of pure or mixed mathematics which his genius has not enriched by memoirs of far-reaching importance. A complete edition of his works has been undertaken by a Swiss commission.

We are here only concerned with his relations with Stirling. Apparently Euler had opened the correspondence by a letter to Stirling, in which he announces, *inter alia*, the theorem known as the Euler-Maclaurin Theorem (Reiff, *Geschichte der Unendlichen Reihen*). This letter is not preserved, but copies

of the letters that passed between Euler and Stirling appear to have been in existence at Petrograd: and Professor Eneström in his *Vorläufiges Verzeichnis der Briefe von und an L. Euler, 1726-41*, furnishes the following dates:

- (1) Euler to Stirling, 9th June, 1736,
- (2) Stirling to Euler, April, 1738,
- (3) Euler to Stirling, 27th July, 1738.

The letters preserved at Garden are doubtless (2) and (3).

It remains to be seen whether the letters in Petrograd have survived the fury of the Revolution in Russia.

Stirling's reply was much belated, for his time was now entirely devoted to the successful development of the Lead Hills Mines, of which he had been appointed manager a year or two before. The rough draft of it is all that Stirling preserved, and is here given with all his corrections and erasures. Stirling acknowledges the importance of Euler's Theorem, and remarks that his own theorem, 'Theorema meum', for summing Logarithms is only a particular case. He informs Euler that Maclaurin has an identical theorem in the proof-sheets of a Treatise of Fluxions to appear shortly. At the same time he offers to communicate Euler's results to the Royal Society, and suggests that Euler should become a Fellow.

With characteristic modesty and absolute freedom from jealousy, Euler in his reply waives his claim to priority over Maclaurin, and proposes that the Royal Society should publish a paper on the *Equation of Riccati*, which he had sent some years before to Sloane the President.

There can be little doubt that Euler and Maclaurin discovered the theorem independently, and the suggestion made by Reiff to call it the Euler-Maclaurin Theorem seems fully justified.

Maclaurin, by the way, does not refer to it in the introduction to his *Fluxions*, but on p. 691 of his Treatise. Euler first gave his theorem without proof in his *Methodus generalis summandi progressionis Comm. Petrop. ad annos 1732, 1733*: published 1738.

The proof is given in *Inventio Summae cujusque seriei ex dato termino generali Comm. Petrop., 1736*: published 1741.

Compare Stirling's letter to Maclaurin I. 15.

I cannot here further discuss Euler's letter, which is almost encyclopædic in its range, save to say that Stirling had shown in his *Meth. Diff.* how to approximate with any desired accuracy to  $\sum_1^{\infty} \frac{1}{n^2}$ , without being aware of its expression as  $\pi^2/6$ .

(See letters of Dan Bernoulli to Euler in Fuss, *Corr. Math.*, &c.) As is well known, Euler became F.R.S. in 1746.

## XII

### M. FOLKES, P.R.S., TO STIRLING

This is the letter of latest date in the correspondence. It conveys to Stirling the news that he had been made a member of the Royal Academy of Science at Berlin, an honour which has not hitherto been noted in any of the biographies of Stirling.

May the Mr. Mitchell who brings the letter to Stirling not have been Maclaurin's friend, better known as Sir Andrew Mitchell, who afterwards became Ambassador at the court of Frederick the Great?

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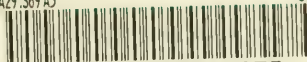


## Date Due

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Stirling, James  
James Stirling; a sketch of his life and

Science QA 29 .S69 A3

Stirling, James, 1692-1770.

James Stirling

